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THE ORIGIN OF VEINS OF THE ASBESTIFORM MINERALS

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In its strict application the name asbestos is limited to the fibrous varieties of the monoclinic amphiboles. The term is commonly applied, however, to those minerals having a highly developed fibrous (asbestiform) structure. When such minerals occur in veins the fibers are parallel, and run transverse to the strike of the veins. Hence they have been called cross-fiber veins.

Microscopic examination shows that bundles consisting of thousands of fibers of chrysotile or amphibole asbestos behave as crystal units, an entire bundle exhibiting the same optical properties as any one of the component fibers. All asbestiform minerals have prismatic cleavage, and this suggests that the fibrous structure may be due to an abnormal development of the cleavage, or at least that the separation of the fibers takes place largely along cleavage planes. The highly fibrous structure of the asbestiform minerals is not, however, a crystallization phenomenon in the sense that it is due solely to the inherent physical properties of the crystal molecule, for all minerals having asbestiform varieties occur in non-fibrous as well as fibrous forms.

A comparative study of the occurrence of all commonly fibrous minerals indicates that the peculiar structure of asbestiform minerals is due to the accentuation of a normal prismatic habit and cleavage by physical conditions which have limited crystal growth to a direction parallel to the principal axis. *Many minerals possessing perfect prismatic cleavage do not have fibrous varieties*, but they are always minerals that crystallize from fusion or solution under conditions permitting of growth in more than a single direction. All asbestiform minerals are second-

ary and for the most part they are limited to metamorphic rocks. The primary minerals of igneous rocks are never fibrous. *Many minerals without prismatic cleavage have fibrous varieties.* They differ from asbestos in having fibers that are usually coarser, more brittle and not so easily separable. With fibers of equal size the flexibility and tensile strength are probably determined chiefly by chemical composition. When minerals do not have a columnar or prismatic habit, the fibrous structure is obviously due to mechanical conditions which have prevented growth except in one direction.

The shape of a growing crystal is controlled by one or more of three independent factors, namely: (1) the tendency to assume a regular polyhedral form because of the forces of surface tension and molecular orientation; (2) the relative and absolute magnitude of the external forces resisting growth in different directions; and (3) the accessibility of the material from which the crystal is built. As a result of the first factor some substances normally crystallize in slender acicular or hair-like forms, but an asbestiform structure is never produced without the assistance of one of the other two factors. Crystal growth under unequal pressure undoubtedly explains the development of fibrous structure in some instances. The solubility of most substances increases with the pressure and therefore crystal growth may be limited to the direction of least pressure. This hypothesis is not applicable, however, to fibers that develop normal to the walls of a vein. Moreover the veins frequently intersect and run in all directions through a given rock mass. Such occurrences of abnormal fibrous structure must therefore be attributed largely to the circumstance that the material for growth was accessible only in one direction.

The efficacy of this method of producing fibrous structure in crystals has been proved experimentally.[1] Cups of porous porcelain were partly immersed in concentrated solutions of copper sulphate, alum, and other salts, thus permitting the solutions to be drawn by capillary attraction into the upper and exposed walls of the cups. When the solutions occupying the small pores were supersaturated by evaporation or cooling so that crystallization began, fibrous crystals grew slowly outward from the surfaces of the cups.

Crystals also developed at favorable places within the walls of the cups, which were ruptured by the pressure as growth continued. The fractures thus formed were gradually extended and widened by the growth of fibrous veins, closely resembling in structure the cross-fiber veins found in rocks. The crystals have a fibrous structure because additions of new material were made only at their base. Using alum

solutions it was found possible to change the color of the material by adding chrome alum, and so prove experimentally that the fibers grew only at their base.

The development of the fibrous structure is probably aided by small adjustments or slips along the surfaces of the fiber prisms and cleavages when the latter are present.

Fibers of copper sulphate and alum are brittle and therefore difficultly separable, but some were obtained that had a length of several millimeters and a thickness of less than 0.001 mm. Such fibers are slightly flexible and somewhat elastic. Slender acicular forms are never found in crystals of copper sulphate or alum that have grown normally in free contact with solutions, because this form is not the most stable. The total surface energy of one of the acicular crystals is great as compared with the mass, and therefore they are dissolved by solutions that are supersaturated with respect to more stable forms.

Many theories[2] have been advanced in explanation of the origin of cross-fiber veins, particularly those of chrysotile. Most of these theories presuppose the existence of open fissures in which the vein minerals were deposited. It is conceivable that some cross-fiber veins may have been formed in pre-existing fissures, but in most cases this is mechanically impossible.

Dresser[3] has pointed out the absurdity of this theory as applied to the chrysotile deposits of southern Quebec, Canada, where the veins run in all directions from vertical to horizontal, occasionally reaching a length of 100 feet, and in places occupy over 10% of the entire rock. According to Pratt[4] and Diller[5] the chrysotile veins in the Grand Canyon, Arizona, over 4000 feet below the rim, extend horizontally parallel to the bedding of the enclosing rocks for distances of 150 feet or more.

In many instances there is evidence that the formation of chrysotile veins and the alteration of the enclosing rock to form massive serpentine were contemporaneous processes; but the alteration of a rock to serpentine is usually accompanied by an increase in volume sufficient to close all appreciable openings. The lenticular shape of many veins is an argument against the theory of deposition in open fissures of mechanical origin.

It has been suggested that chrysotile veins may have been formed by some process of replacement, but no one has explained why serpentine should replace serpentine of the same chemical composition. Moreover, chrysotile veins never contain pseudomorphs or show any trace of an inherited structure. Replacement veins are characterized by great irregularity in width and lack of definite boundaries, while chrys-

otile veins are sometimes remarkably uniform in width, and always have well defined walls that are easily separable from the veins.

The last objection applies likewise to the hypothesis that chrysotile veins represent portions of the serpentine that have crystallized *in situ*. Recrystallization is a common phenomenon in rocks, but it is never confined to a narrow vein-like zone with sharply defined walls. If chrysotile can be formed by such a process, why is the ratio of chrysotile to massive serpentine limited? Why is the entire mass of serpentine never recrystallized to form chrysotile?

Not one of the theories previously cited satisfactorily explains the presence of one or several partings in some veins and their absence in others. They do not explain the angular inclusions of massive serpentine that frequently mark the partings in chrysotile veins and are also found irregularly distributed through them. They do not explain why the fibers are normal to the vein walls in some instances and oblique in others. They do not explain the presence of sharp bends in the fibers, nor veins of cross fiber that grade into slip fiber. In view of all these facts it must be concluded that previous theories are inadequate to explain the origin of fibrous veins.

As noted above, cross-fiber veins with structural features similar to those found in rocks have been produced in the laboratory where their formation and growth could be observed. The evidence thus obtained supplements that furnished by veins of fibrous minerals, and makes inevitable the conclusion that cross-fiber veins are formed through a process of lateral secretion the growing veins making room for themselves by pushing apart the enclosing walls.

The force that enables the growing veins to push apart their walls is not due to the tendency of a crystalline substance to assume a regular polyhedral form, for the columnar or fibrous structure of most minerals occurring in cross-fiber veins is not a crystallization property, but is caused by the conditions of growth.[6] Under similar conditions the fibrous structure will develop in substances that crystallize in any of the systems of crystallization. In most cases it is not the normal habit, and therefore is unstable. The writer believes that the force is due chiefly to the expansion in volume which accompanies the separation of most solids from solution, for, as yet, he has obtained no pressure effects during the crystallization of substances that separate from solution with decrease in volume. When a substance separates from solution with increase in volume, the pressure developed depends on the magnitude of the forces resisting expansion, and may be much greater than the force required to crush the substance. It is improbable that pressure alone

can expel solutions occupying subcapillary pores in rocks; and in serpentine and other rocks found enclosing cross-fiber veins the openings are almost entirely subcapillary in size. In such cases the transfer of material to the growing vein is probably due to diffusion rather than to circulation.

A central parting is formed whenever fibers start to grow from both walls of a fracture, and apparently this always happens when veins develop along pre-existing fractures unless growth is limited to one side only. In laboratory experiments, growth is frequently limited to one side by the development of the fracture in such a direction as to cut the other side off from solutions furnishing material for growth. When the formation of the fracture and the beginning of vein growth are contemporaneous, the parting is absent because of simultaneous growth at both ends of the fibers—a fact proved in the laboratory by changing the color of the solutions so as to produce symmetrically banded veins. Partings may also result from a pause in the process of growth or from a slight displacement of the walls. The irregularity of some partings is due to the more rapid growth of groups of fibers most favorably situated to receive additions of new material.

The inclusions of wall rock found along central partings represent fragments formed when rupture occurred, and their position is due to the growth of the vein on both sides as new material was added through the walls. Occasionally vein matter begins to crystallize along an incipient fracture or line of weakness close to the vein, and in this way a fragment is gradually separated from the wall and included in the growing vein.

The fibers are always parallel to one another and extend in the direction in which the walls moved as they were pushed apart by vein growth. In most veins the fibers are normal to the walls because the latter have been forced directly apart, but when the walls have had also a lateral displacement because of the simultaneous growth of adjacent non-parallel veins or other causes, the fibers grow in the direction of the resultant motion. If the course of the vein is not straight, the fibers may be normal to the walls at one place and oblique at another. As long as the relative motion of the walls of a growing vein is in a straight line, the fibers are straight; any change in the direction of motion is immediately recorded by the slowly lengthening fibers. If the change in the direction of relative motion is gradual and continuous the fibers are curved; if abrupt, it results in the development of sharp bends. Sometimes the fibers of a chrysotile vein record several changes in the relative movement of the walls, and this gives a banded

appearance due to the unequal reflection of light where the fibers run in different directions.

The structural phenomena discussed in the preceding paragraphs all furnish evidence tending to confirm the writer's theory of the origin of cross-fiber veins, and they are difficult or impossible of explanation under any other hypothesis of vein formation hitherto advanced.

Conclusions. Cross-fiber veins are formed through a process of lateral secretion, the growing veins making room for themselves by pushing apart the enclosing walls. The fibrous structure is to be attributed largely to the mechanical limitation of crystal growth through the addition of new material in only one direction. In the case of the asbestiform minerals the fibrous structure is accentuated by a normal prismatic habit and cleavage.

[1] The experiments herein referred to are described in my more extended paper on "The Genesis of Asbestos and Asbestiform Minerals" which will appear in an early number of *Bull. Amer. Inst. Min. Eng.*

[2] For a review of these theories, see Fritz Cirkel, Chrysotile asbestos, its occurrence, exploitation, milling and uses, Ed. 2, *Canada Dept. of Mines, Mines Branch, Report No. 69* (1910); O. B. Hopkins, A report on the asbestos, talc and soapstone deposits of Georgia, *Geol. Survey Ga., Bull.* No. 29 (1914); and J. A. Dresser, Preliminary report on the serpentine and associated rocks of Southern Quebec, *Canada Dept. Mines Geol. Survey Mem.* No. 22 (1913),

[3] Dresser, J. A., *Ibid.*, p. 65.

[4] Pratt, J. H., Asbestos, *U. S. Geol. Survey Mineral Resources*, 1904, pp. 1137–1140 (1905).

[5] Diller, J. S., Asbestos, *U. S. Geol. Survey Mineral Resources*, 1907, pt. II, pp. 720–721 (1908).

[6] Taber, Stephen, The growth of crystals under external pressure, *Amer. J. Sci.*, ser. 4, **41**, 532–556 (1916).

A NEW TEST OF THE SUBSIDENCE THEORY OF CORAL REEFS

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According to Charles Darwin's much-discussed theory, a barrier reef represents the upgrowth of a coral reef which originally fringed a sinking (generally volcanic) island; and an atoll reef represents a further upgrowth, completed in typical form after the central island has sunk below sea-level. Between the upgrowing reef and the subsiding, central island is a concavity or 'moat.' Through the accumulation of detritus and shells and skeletons of organisms inside the reef, the moat is slowly filled. Above the detrital covering of the moat surface is the water of the 'lagoon.' The subsidence is supposed to have progressed

until the deepest part of each moat has sunk scores, hundreds, or possibly thousands, of meters (see fig. 1).

The observed general flatness of the lagoon floors of atolls shows that there the hypothetical moats must have been completely filled and obliterated as distinct submarine forms. The depression occupied by the lagoon water inside each barrier reef has the position of the hypothetical moat, but in every case the lagoon floor is about as flat as an atoll-lagoon floor. For each kind of lagoon, therefore, the filling of the moat

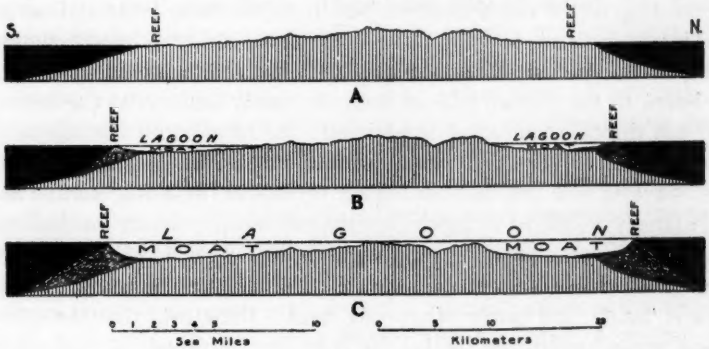

FIG. 1

Sections illustrating the subsidence theory as applied to the wider barrier and atoll lagoons; also illustrating the relations of moat, moat filling, lagoon, and central island. There is no exaggeration of the vertical element, and the depth of water in the deepest existing lagoons is no greater than that shown, to scale, by the width of the black line drawn below the word 'Lagoon.' Water indicated by solid black.

A. North-south section of the island of Kauai, chosen as a large, deeply eroded, volcanic island, which may be fairly presumed to typify the ancestor of one of the larger atolls, according to the subsidence theory. Fringing reefs indicated.

B. Section of the same island after subsidence, with the growth of each fringing reef (now become a barrier) upward and outward on its own talus (*T*). Between the reef and central island is the moat, the upper part of which is occupied by the water of the lagoon. The detrital filling of the moat is not shown.

C. Section of the same island after further drowning, with widening of the moat and ultimate development of an atoll.

must be assumed to be far advanced. The inferred maximum depths of calcareous detritus are to be estimated in scores, hundreds, or possibly thousands, of meters. The mechanism of this filling has never been adequately discussed; yet its analysis cannot fail to indicate a valuable test of the subsidence theory of reefs. To the writer's knowledge, no earlier statement of the test has been published.

The filling mechanism involves two factors: sediment, and active transportation of that sediment. Neither has been thoroughly discussed by Darwin or by any of his supporters; yet serious attention to

both factors is the manifest, special duty of all those who advocate the subsidence theory.

Darwin stated that the filling of the concavity, here called a moat, is to be referred to "the growth of the delicately branched corals within the reef, and to the accumulation of sediment" (*The Structure and Distribution of Coral Reefs*, London, 2d ed., 1874, p. 132). He did not precisely indicate the nature or origin of the sediment. In part it is derived from the erosion of the central island, though at a rate rapidly diminishing as the island sinks; in part from the reefs, inner and outer; and in part from the remains of non-coral organisms, both free-swimming and bottom species. The rain of planktonic and nektonic shells and skeletons to the bottom will, of itself, be nearly uniform in the lagoon and not directly competent to obliterate the moat, until the lagoon is filled almost to sea-level. Modern field studies, like those at Funafuti, have shown that bottom lagoon organisms, including corals, are very largely restricted to depths less than 30 meters; that is, to shallows immediately adjoining the main reef, the coral knolls of the lagoon, and the shores of the central island, if present. In general the lagoon floor is 'sandy' and barren of growing coral. Dana held that the open-ocean waves throw into the lagoon, over the main reef, 'a large part' of the débris eroded from the growing, outer face of that reef. However, the detritus thrown over the main reef by waves of the open ocean is practically nil in all those cases where the reefs form dry land; and there are evidences, not here to be detailed, that the relative amount of lagoon sediment so added to the deposits in the average lagoon is actually small. The sediment brought into the lagoon through the occasional ship channels may be more important, but most of it is deposited, in delta form, just inside the main reef.

Thus, whatever be the various origins of the lagoon sediments, the detritus necessary to fill the hypothetical moat must be found principally in the shallower places within the lagoon. If the detritus were as fine as mud, its transfer to the deeper part of the moat might be comparatively rapid. Observation shows, on the other hand, that lagoon floors are characteristically more sandy than muddy, even in depths of 50 meters or more. Marine sand is transported on the bottom. It follows, then, that, if the floor of the hypothetical moat were carried down by subsidence to depths greater than those where waves and currents can stir bottom sands, the moat could be obliterated only after the detrital shelves, advancing from the shallows, have become confluent. Obliteration of the moat by this process is extremely slow for narrow lagoons, still slower for lagoons 20 to 50 kilometers wide.

Incidentally a question, not directly the subject of the present communication, arises. Is it possible to credit this explanation of all the hundreds of atoll lagoons, every one of which exhibits no trace of the hypothetical moat? The entire absence of moat concavities in the world's atolls and the correlated flatness of barrier lagoons can only mean prolonged crustal stability in the reef-charged areas.

The agents most responsible for the transportation of sediment into the depths of the hypothetical moat must be the waves and currents set going in the lagoon itself. The normal atoll or barrier reef, which reaches nearly or quite to sea-level, almost perfectly protects the lagoon from the powerful waves and currents of the open ocean. Some water is thrown over into the lagoon when great rollers break on low parts of the reef, with the effect of forming currents in the lagoon; but such currents cannot be other than weak. Tidal currents may be strong in narrow ship channels; inside the reef they are weak. The advance of detrital shelves, so as to fill the hypothetical moat, must, then, be very largely attributed to the activity of waves and currents originating in the lagoon. Except possibly during the rare hurricanes, these agents are incompetent to brush sand over the general bottom at the depths commonly ruling in atoll and barrier lagoons. But, assuming (in accordance with the hypothesis) that the transporting agents have been competent to fill the moats, an important effect on the form of the lagoon floor should be expected; the moat might in time be nearly filled, but *the lagoon floor should not be level.*

That feature ought to be found especially in lagoons situated in the trade-wind belts and not affected by monsoons or other strong winds from westerly quadrants. In these cases the lagoon waves and currents must set in a westerly direction, and chiefly in that direction sediment should be transported. Hence the hypothetical moat, like its existing lagoon remnant, should be filled at different rates on the windward and leeward sides respectively. According to the degree of enclosure of the lagoon by continuous reefs, the leeward part of its floor should now be higher or lower than the windward part. To use a technical expression, the lagoon area should be differentially aggraded. The same tendency ought to be seen in lagoons subject to variable winds, among which is a dominant system of winds blowing from one quarter of the compass.

To this reasoning it might be objected that back-running currents would reverse the direction of transportation for bottom débris. 'Banking' of water against the leeward section of the main reef would naturally cause return currents, if the reef were there continuous. Even in that case the currents must be comparatively feeble and unable

to stir sand at depths of 40 or more meters. However, the leeward part of each atoll and barrier reef is usually discontinuous, the gaps in the reef wall being deep enough and wide enough to permit the ready escape of water blown thither by the wind. Hence, return or compensatory currents are not likely to be efficient in undoing the work of the direct, wind-driven waves and currents.

Consulting the charts and meteorological records, one finds scores of typical atolls located in belts where the trade winds cause the dominant waves and currents. The floor of each of these lagoons is strikingly level and not higher, more aggraded, either to leeward or to windward of the lagoon center. An example is the best known of all atolls, Funafuti, in the Ellice group of the Pacific (fig. 2). Funafuti has occasional strong winds from westerly quadrants, but records show the dominance of the trade winds. Since the main reef at Funafuti is continuous for long stretches on the leeward side, its lagoon should, according to the subsidence theory, be distinctly shallower there than on the windward side. The charted soundings, actually much more numerous than those entered in figure 2, demonstrate that the floor of the lagoon is nearly level and that the variations in depth have no systematic relation to the trend of the trade wind.

The lagoon floors of many other Pacific atolls, including those of the Marshall and Paumotu groups, show similar levelness. Most barrier lagoons of the Fiji and other archipelagoes in the trade-wind belts are also characterized by practically the same depths to windward and leeward of their respective central islands. Exceptional lagoons in the southwest Pacific have been shallowed or deepened, uniformly or differentially, by recent, local warpings of the earth's crust; in these instances the test cannot be applied with conclusiveness. Nor is it easily applied to the Maldives and neighboring atolls of the Indian ocean, where the monsoons change direction, through an angle of 180°, every few months.

Nevertheless, in spite of the quite special weather conditions required by the test, very many atoll and barrier lagoons are known to be favorably situated, and yet none shows systematic lack of symmetry in the submarine profiles crossing its lagoon from windward to leeward. So also are the cross-profiles of the windward and leeward reefs themselves respectively of the same quality. The windward reefs tend to be more developed, but their inner slopes are of nearly the same steepness as the inner slopes of the corresponding leeward reefs.

In conclusion, observed facts do not seem to agree with a legitimate deduction from the subsidence theory of coral reefs. On the other

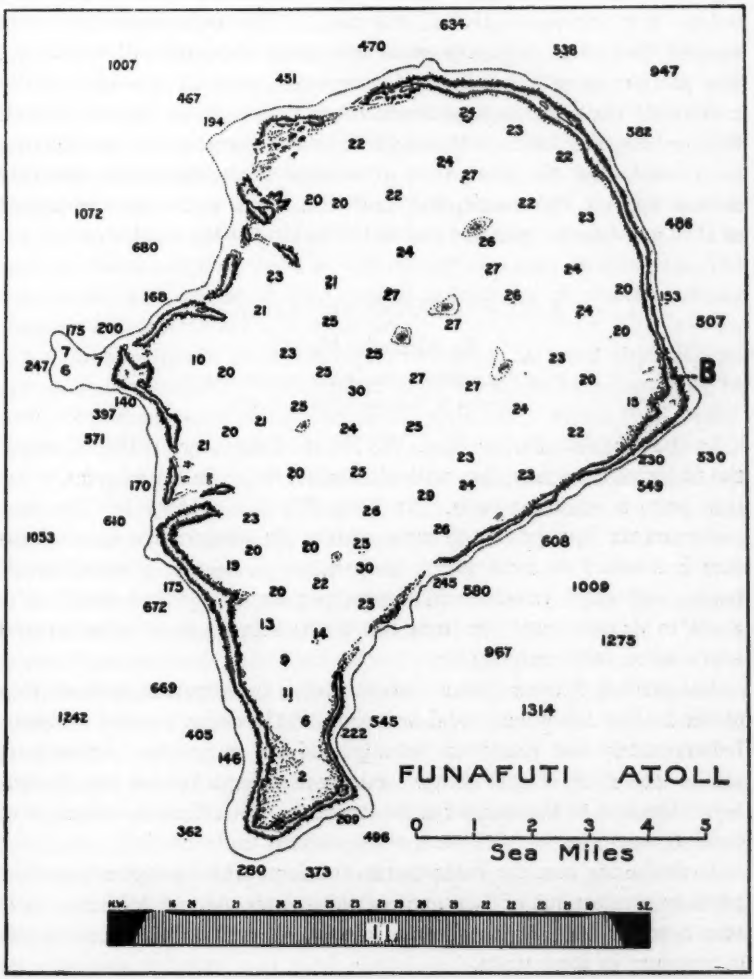

FIG. 2

Chart and section of the Funafuti atoll. Depths in fathoms (1 fm. is 1.83 m.). In the section water is shown in solid black; vertical scale three times the horizontal. The section is run near the middle of the atoll and in the general direction of the trade wind.

hand, a study of the symmetrical profiles of the reef-lagoon structures here considered has furnished one more direct indication that the existing coral reefs are new upgrowths on platforms, which have been formed before, and independently of, the reefs. The volumes of the reefs suggest that these began to grow not many thousands of years ago. The platforms, on which the reef crowns rest, seem to have been finally smoothed, and prepared as foundations, during the Glacial period. In any case, the submarine topography of each reef-platform structure *as a whole* and the elementary principles of oceanography together declare against the assumption that the forms and spatial relations of atoll and barrier reefs are due to the sinking of the earth's crust.

A NEW THERMOMETER SCALE

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In the *Physical Review* (Vol. VI, No. 6, December, 1915), I urged the importance of adopting without further delay, in aerophysics, units that have a scientific basis. At Blue Hill Observatory for the past two years we have published summaries of air pressure also vapor pressure in *kilobars* or force units, temperature in degrees absolute centigrade, and wind velocities in meters per second. Wind direction is given in degrees, starting from the West; rainfall in millimeters and evaporation in force units.

The British Meteorological Office, under the progressive leadership of Sir Napier Shaw has published since 1915 results in similar units. Unfortunately the European definition of the megadyne atmosphere, the *bar* so-called, is misleading, a more scientific definition being *megabar*. The use of the smaller unit does away with the inconsistency of defining a millibar as 1000 bars, a self-evident contradiction, and allows us to define the bar, the basic unit as the force which would impart to a gram an acceleration of 1 cm. per second per second. A *kilobar* or 1000 bars becomes then the natural and consistent unit for all measurements of pressure in force units.

With regard to temperature, a concept all important in atmospherics, it is regrettable that we have no available method of expressing gain or loss of heat in terms of molecular motion. The ordinary mercurial thermometer is certainly a crude instrument and so far as thermometer scales go, there has been no real improvement since Linnaeus reversed the order of the Celsius marking. It will be generally conceded that

the Fahrenheit scale has now outlived its usefulness. Some meteorologists however still oppose the use of the centigrade scale and also the absolute, on the ground that the scale division even when read to tenths is too large for meteorological purposes.

To meet this objection and for other reasons, I suggest a new scale to be known as the New Absolute, or briefly, New. The zero of the new scale will be the same as that of the absolute, approximately 491° below freezing on the Fahrenheit and 273° below on the centigrade. The other fiducial point is the temperature of melting ice at a pressure of 1000 kilobars and is marked 1000. (The degree sign is omitted as it has been decided to reserve this symbol for angular measure.) The scale divisions are thus 0.366 of the centigrade, even smaller than the Fahrenheit and permit of any desired refinement of reading. Some other advantages of the new scale are:

1. The abolition of all minus signs. In upper air work temperatures are far below freezing. At a height of 10 kilometers readings may be lower than those recorded by Scott in the Antarctic. Given such a reading as −66.0°F., or −54.0°C or 219.0°A.; on the new scale this is read 800. Or again, some of the surface winter temperatures are confusing unless the minus sign is emphasized. The new scale has an advantage here as when we write 900 in place of −17.5°F. or −27.0°C or even 245.7°A.

2. The grand division of *warm* and *cold* as experienced by the general public in the every-day affairs of life is characteristically marked. Warm is any reading above 1100; cold is any reading below 1000.

3. In published tabular work there is a saving in typographical composition. There is also a saving of time in computation and increased accuracy.

4. But of even greater importance is the fact that the new scale makes for clearer conceptions of the nature and magnitude of temperature changes. It is astonishing how indefinite and vague are the ideas of most students regarding heat and the significance of a given temperature. And furthermore it is a difficult matter with the present scales to present clearly to the student a picture of the process of energy transfer available as heat.

5. The new scale starting as it does from the temperature of no molecular motion and laying stress upon the temperature of change of form of the most familiar substance, water to ice, has it would seem to me, a certain educational value, which the Fahrenheit and centigrade certainly do not have and which the absolute scale owing to the awkwardness of the fraction 1/273 loses.

6. Finally in problems of thermodynamics, there would seem to be no valid objection to its use. In the characteristic equation for a pure gas, the product of the new reading and the gas constant give as before a measure of the kinetic energy of translation of the molecules. Similarly in Avogardo's law, we have an even more definite conception of the temperature function; and also in the Stefan-Boltzmann law of black body radiation.

ON THE IMMUNITY COLORATION OF SOME NUDIBRANCHS

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Certain large nudibranchs, notably species of *Chromodoris*, exhibit a combination of brilliancy of pigment pattern and boldness of habits sharply distinguishing them from related nudibranchs which appear to be efficiently protected by their concealing coloration and behavior. It has long been suspected that the strikingly colored chromodorids achieve an immunity from predatory enemies by virtue of some distasteful quality, according to the classical conception of 'warning coloration.' The experimental evidence for this belief, as presented by Herdman and by Crossland, has remained, however, very fragmentary and is indeed far from carrying conviction.

For several years I have been interested in the pigmentation of the large Bermudian *Chromodoris zebra* Heilprin, mainly because its blue integumentary pigment is available as an intracellular indicator of acidity. On the basis of field observations, continuous throughout the year, I was led to undertake a series of experiments intended to throw light upon the possible biological meaning of this animal's coloration. The outcome seems reasonably conclusive, and it leads to the rejection of the idea that a warning significance attaches to the pigmentation of *Chromodoris*. By inference, one is inclined to suspect that more complete study will reveal analogous conditions with reference to other brightly colored nudibranchs.

The pigment pattern of *C. zebra* is, in brief, an irregular streaking of yellow or orange upon a field of blue. The foot, the ventral surface of the mantle, and various less conspicuous parts are of a pure blue. The animals occur at various stations, ranging in depth from about 10 fathoms to the surface of the water. They are most abundant, though, in quite shallow water, except during a portion of the summer and immediately after severe storms at other seasons. Their color-

*Contributions from the Bermuda Biological Station for Research, No. 51.

ation is always conspicuous, not only because of its brilliancy but also by reason of the freedom with which they expose themselves in bright sunlight upon bare open bottoms or (a favorite situation) on eel grass in tidal 'creeks.' The animals are positively phototropic. Their conspicuousness is entirely independent of their being viewed form above the water surface.

Less than two per cent of the specimens collected are found to show evidence of mutilation. As these nudibranchs regenerate very slowly (Child, and my own observations), this evidence is significant.

Experiments in feeding entire, undamaged specimens of this nudibranch to other animals, both in natural surroundings and under favorable conditions in aquaria, have shown that to all the animals usually associated with *Chromodoris* its flesh is repugnant. This is true of fishes as well as invertebrates, including anemones, crustacea, various worms, and starfish. Some fishes which feed by night (squirrel fishes), and others which hunt their prey by sight (snappers), will make several attempts to bite a *Chromodoris* when it is first dropped into their aquarium tank, but thereafter will not approach it; after the nudibranch has sunk to the bottom and begun its normal creeping, it is never seriously molested.

The unpleasant quality is associated with the skin, since the internal organs are greedily devoured. The blue pigment is not the unpleasant substance, for the intensely blue rhinophores are eaten readily, while the blood, which I have elsewhere shown to contain the blue skin pigment (presumably as a respiratory chromogen of some sort), evokes positive food-taking responses from various fishes, crabs, and anemones. The yellow pigment is not the responsible agent, as it is absent from the edge of the mantle, a region which comparative tests indicate to be the most 'unpleasant' part of the animal's body.

Small pieces of the skin of *Chromodoris* will be snapped at several times by a fish (over twenty species have been tested), while an intact nudibranch will usually be merely approached, 'nosed,' and perhaps bitten once, if the fish swims up suddenly, before it is left alone. The explanation of this behavior is found in the fact that when the intact nudibranch is locally disturbed by being handled severely, bitten, cut, stung by nematocysts, or stimulated with induction shocks, it excretes from the irritated area, into the sea water, a bluish-white material which causes immediate negative reactions in fishes and in all classes of marine invertebrates which I have tested. Isolated portions of the *Chromodoris* skin, however, give the reaction with difficulty, if at all, and then only to a slight extent.

It is significant that the secretion comes mostly from the ventral surface of the mantle. When this nudibranch is disturbed, its first reaction is a general contraction, in which the mantle edge is extended and made prominent by internal fluid pressure; the edge of the mantle is invariably the region which is bitten at by fishes, and it is only the part which is damaged in the small number of mutilated specimens found in the field. There exists here, I believe, an interesting case of adaptive correlation.

As excreted, the repelling material contains globules of the blue skin pigment, which seems to be accidentally included in the discharged substance; but it is mainly a coagulated white substance holding oily globules. It is this substance which is the essential repellant.

There is still another phase of the matter, which is the most significant of all. A striking characteristic of these nudibranchs is the curiously penetrating, disagreeable odor which attends them even when they are undisturbed. To this is to be assigned responsibility, I believe, for the facts, (1) that cloth bags containing *Chromodoris* are avoided by fishes; (2) that blinded fishes avoid them; and (3) that animals never normally in contact with *Chromodoris*, such as the dogfish, remora, and the hawksbill turtle, will approach this nudibranch closely, then quickly retreat without touching it. The olfactory locus of these reactions seems probable, but it does not necessarily involve the idea of 'warning odor.'

In the present instance we have, in my opinion, positive proof for the validity of the theory of immunity coloration developed by Reighard. According to this view, startling colors are in many (if not in most) cases "conceived to have arisen through internal forces under immunity of the organism from the action of selection on its characters." The immunity of *Chromodoris zebra* is determined by the repugnatorial character of skin secretions under the control of its nervous system, and is further made effective by a repelling odor, which we have reason to believe stimulates the olfactory organs of fishes at least. Its success in maintaining itself as a plentiful inhabitant of the shoal waters of a 'coral' reef region is conditioned by this immunity and by the unpalatable nature of the jelly surrounding its egg strings; the slow rate of development of its eggs is counterbalanced by the fact that the animals reproduce throughout the year.

The coloration of *C. zebra* is a metabolic accident, at least in relation to its protection, for a single experience with a normally colored specimen is sufficient to cause snappers, turbots, and groupers to have nothing to do with subsequent individuals offered to them, even though these individuals are stained red or blue.

Conceivably, its color may at times serve to warn predatory foes, but the evidence herein adduced makes it clear that the pigmentation of *Chromodoris* plays no necessary rôle of this kind. We are consequently at liberty to infer that the conspicuous coloration did not develop, as the result of selection, according to the scheme proposed by the theory of warning coloration.

An illustrated account of this work will be submitted for publication later.

SOME EFFECTS OF THE CONTINUED ADMINISTRATION OF ALCOHOL TO THE DOMESTIC FOWL, WITH SPECIAL REFERENCE TO THE PROGENY

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1. In a recent number of these PROCEEDINGS there was presented[2] a summary statement of the results which had been obtained up to February 1, 1916, in a series of experiments designed to test the effect upon the progeny of the continued daily administration, by the inhalation method, of either ethyl alcohol, methyl alcohol, or ether to the parents, the domestic fowl being the form used in the work. The chief result was that the offspring of parents which had inhaled the vapor of one or another of the substances mentioned, for one hour each day over an average period of 210 days each, were measurably *superior* to the offspring of untreated parents in respect of the following characters or attributes: prenatal mortality, postnatal mortality at all ages, weight at hatching, adult weight, rate of growth, and percentage of abnormalities or defects of development. The offspring of untreated parents were superior to those of treated parents only in respect of their number; the untreated parents produced a larger total number.

It is the purpose of the present paper to report in brief and condensed, but numerical, form the results which have been obtained since the data at which the former report ended. The data included are those obtained in the breeding and rearing season of 1916. The length of the period over which the parents had been given daily treatments was of course longer by one year than in the 1915 breeding season. Thus the total germ dosage index[3] for the 1916 matings ranged from 458 to 1138, with a mean value of 562.9 days, for the parental generation, and from 0 to 266, with a mean of 40.6, for the grandparental generation.

Owing to various reasons, which cannot be gone into here, ether has been dropped from the list of experimental agents, ethyl and methyl alcohols alone being used now.

A detailed account of the experimental methods used and the precautions taken to ensure accuracy and trustworthiness of results is given in the complete reports now in press.

2. The general results of the 1916 matings, so far as concern the chief characters which may be taken as indices of vigor and vitality, are set forth in Table 1.

TABLE 1

SHOWING THE MORTALITY (PRENATAL AND POSTNATAL) OF CHICKS OF ALCOHOLIC ANCESTRY, AS COMPARED WITH THOSE OF NON-ALCOHOLIC ANCESTRY

Matings in 1916

NATURE OF ANCESTRY	NUMBER OF MATINGS	TOTAL EGGS SET	TOTAL ZYGOTES FORMED	ZYGOTES FORMED	PRENATAL MORTALITY	POSTNATAL (180 DAY) MORTALITY	ZYGOTES ALIVE AT 180 DAYS OF AGE
				per cent	*per cent*		*per cent*
Both parents alcoholic	5	82	18	21.95	11.11	25.00	66.67
Dam only alcoholic	3	63	5	7.94	80.00	0	20.00
Sire only alcoholic	13	512	274	53.52	47.08	13.79	45.62
Sire and one or more grandparents alcoholic	3	233	158	67.81	46.84	28.38	39.87
One or more grandparents alcoholic	3	187	113	60.43	46.02	6.56	50.44
All of alcoholic ancestry	27	1077	568	52.74	45.95	16.50	45.42
All of non-alcoholic ancestry	28	1333	1060	79.52	55.94	21.20	34.72
Differences	*1*	*256*	*492*	*26.78*	*9.99*	*4.70*	*10.70*
Alcoholic better = + Alcoholic poorer = −		−	−	−	+	+	+

From this table we note the following points:

a. Considering the totals, and comparing chicks with any alcoholic ancestry, as a group, with chicks having no alcoholic ancestry, as a group, it is clear that there are fewer of the former than of the latter from a given number of eggs. In every 100 eggs from birds in the alcoholic series approximately 53 zygotes (embryos) were formed as against 80 in every 100 eggs in the non-alcoholic series.

b. The total number of offspring zygotes dealt with in the experiments (1628) is respectable, and such as to lead to reasonable confidence in the results, especially in view of the fact that these results are in every essential particular in full accord as to their sense with

those obtained in the 1915 experiments, and even more pronounced in degree.

c. The prenatal mortality is approximately 10%, and the postnatal mortality (to 180 days of age) is approximately 5% *lower* in the chicks with alcoholic ancestry than in those of non-alcoholic ancestry.

d. While from a given number of eggs 26.78% *fewer* zygotes are formed in the alcoholic series than in the non-alcoholic, 10.7% *more* of those zygotes are alive when adult age (180 days) is reached if their ancestry was alcoholic than if it was non-alcoholic.

e. The great reduction in zygote formation (partial sterility) observed in the alcohol treated series is preponderantly due to the effect of the alcohol upon the germ cells of the *female.* While there is some diminution in the effective fecundity of the male as a result of the treatment, it is much smaller than in the female. In the first two lines of the table, where the dam is alcoholic, we have 22 and 8% of the eggs forming embryos, as against values of from 54 to 68% where the sire is alcoholic and the dam not. The lower value for the percentage of zygote formation in the case of matings where the dam only is alcoholic as compared with those in which both parents are alcoholic, almost certainly does not represent a significant difference, but is an accidental result of the small number of matings in the class. The significant thing is the fact that the percentage of zygote formation steadily rises as we compare the following groups: (1) dam alone or dam and sire alcoholic, (2) sire alone or sire and some other ancestry not including the dam alcoholic, (3) no ancestry alcoholic.

3. In the season of 1916 every fertile egg in which the embryo died on or after the tenth day of incubation was opened, and an examination was made of the embryo in each case, in order to determine whether any structural abnormality or malformation was present. It was not found practicable to deal with embryos under ten days of age. A detailed record was kept of all such congenital abnormalities, in both the dead embryos and hatched chicks. Every discernible departure from perfect structural normality was taken account of, the range including at one extreme such slight and unimportant things as the congenital absence of one toenail in an otherwise perfectly normal chick, and at the other extreme acraniate and eyeless monsters. A later complete report will give the detailed data for the different classes of abnormalities. Here I wish simply to report the general results. These are set forth in Table 2.

TABLE 2

SHOWING THE NUMBER AND PROPORTION OF CONGENITAL ABNORMALITIES AND MALFORMATIONS FOUND AMONG THE CHICKS OF ALCOHOLIC AND NON-ALCOHOLIC ANCESTRY IN THE SEASON OF 1916

OFFSPRING OF	NO. OF MATINGS	MEAN GERMINAL DOSAGE INDEX		DEAD IN SHELL AFTER THE TENTH DAY OF INCUBATION		KILLED AT ABNORMAL HATCH	HATCHED CHICKS		TOTAL ZYGOTES SURVIVING TO THE TENTH DAY OF INCUBATION AND BEYOND	TOTAL ABNORMAL	PER CENT ABNORMAL
		P.	G. P.	Normal	Abnormal		Normal	Abnormal			
Treated parents or grandparents or both.....	27	562.9	40.6	78	19	4	267	20	397	43	*10.8*
Untreated control parents.....	28	0	0	190	23	14	372	42	641	65	*10.2*

From this table the following points should be noted:

a. The apparent discrepancy between Tables 2 and 1 in the figures for total zygotes arises from the fact that in Table 2 account is taken only of zygotes which survived for 10 or more days after fertilization. Out of a total of 568 zygotes formed in the alcoholic ancestry series 171 died before the tenth day of incubation.

b. The proportionate number of abnormalities of development is substantially identical in both series, amounting to about 10% of all zygotes formed. If this seems a high proportion let it be remembered that the category of abnormality is here very broadly defined and includes a number of developmental deviations which in no way reduce the survival value of the individual possessing them.

c. It is entirely certain that in these experiments there was *no significantly higher proportion of abnormalities of development among the zygotes of alcoholic ancestry than among those of non-alcoholic ancestry.* This result is in striking contrast to those which have been reported by Stockard,[4] and would appear to indicate a rather fundamental difference between guinea-pig and fowl germ plasms in respect of their susceptibility to alteration by alcohol.

d. While lack of space prevents the presentation of the detailed evidence here, it may be stated that an analysis of the data shows that the average relative gravity or intensity of the 10.8% of developmental abnormalities observed among the zygotes of alcoholic ancestry is probably not significantly different from that of the 10.2% in the non-alcoholic series.

4. It has been suggested in objection to all experiments where alcohol is administered by the inhalation method used by Stockard, MacDowell, the present writer, and others, that the animal gets no ap-

preciable amount of alcohol into the system. In view of all the facts this position is an extraordinary naive one and could only be held by one who had had no experience with the administration of alcohol by the inhalation method. It is true that it is practically impossible to induce by the inhalation method in animals habituated to alcohol that state of muscular incoördination which is usually, but by no means always, the most striking objective symptom of the condition of being drunk. On the other hand, it is extremely easy to kill an animal with ethyl or methyl alcohol administered by the inhalation method, quite regardless of whether it is or is not habituated to the substance. If the animal 'gets no appreciable amount of alcohol in its system' in the course of one hour's sojourn in a tank containing alcohol vapor it is extraordinarily difficult to understand how it manages to accumulate a fatally toxic dose of alcohol by staying in the same tank under the same conditions for from 20 minutes to half an hour longer. This is not only true for my fowls, for which the above time relations hold, but also, with other time relations, for mammals, as lately shown by Tyson and Schoenberg[5] in the case of methyl alcohol. Thirty-eight years ago Poincaré[6] demonstrated extensive and grave lesions in animals subjected to the continuous inhalation of methyl alcohol vapor.

It appears to be impossible, in the present development of the technique, to state the exact dosage per kilogram of body weight of alcohol administered by the inhalation method. I am now working on an indirect method of measuring the dosage, logically based upon the same reasoning as is used in the so-called 'physiological assay' of drugs. As a first contribution in this direction I wish to present some data regarding the immediate physiological effect of the administration of ethyl and methyl alcohol to fowls thoroughly habituated to these substances. These observations have to do with respiration rate (respirations per minute) and rectal temperature (in degrees Fahrenheit). They are set forth in Table 3. Owing to lack of space this table gives mean values only, observations will be reported in a complete paper later. The means undoubtedly understate the true differences between the different groups because they include all the observations, some of which were made on cold days and before the apparatus for maintaining a constant temperature for the evaporation of the alcohol had been perfected. On such days the dosages, and in consequence the physiological differences, were reduced. By 'tanked controls' in the second line is meant birds which were put for one hour in a tank precisely like the alcohol tanks, but in which there was no alcohol or alcohol vapor.

TABLE 3

SHOWING THE EFFECT OF THE INHALATION OF ETHYL AND METHYL ALCOHOL VAPOR ON THE TEMPERATURE AND RESPIRATION OF FOWLS HABITUATED TO THESE SUBSTANCES

GROUP AND TREATMENT	RECTAL TEMPERATURE BEFORE TREATMENT F°	RECTAL TEMPERATURE AFTER TREATMENT F°	DIFFERENCE	RESPIRATION PER MINUTE BEFORE TREATMENT	RESPIRATION PER MINUTE AFTER TREATMENT	DIFFERENCE	NUMBER OF BIRDS IN GROUP	NUMBER OF OBSERVATIONS
Mean of normal controls..... ..	107.14			23.77			12	48
Mean of tanked controls.....	107.07	107.13	*+0.05*	22.89	23.67	*+0.66*	4	16
Mean of all ethyls.....	107.07	106.43	*−0.64*	18.24	27.86	*+9.62*	7	29
Mean of ethyl ♀ ♀.....	107.00	106.36	*−0.64*	18.24	28.76	*+10.52*	6	25
Mean of all methyls.....	107.14	105.91	*−1.23*	18.56	22.50	*+4.31*	4	16
Mean of methyl ♀ ♀.....	107.53	106.45	*−1.08*	19.00	23.75	*+4.75*	3	12

This table shows that:

a. Those birds which, by long continued daily treatment, extending over periods varying from six months to two years, have become thoroughly habituated to alcohol have a *slower respiration rate* than normal untreated birds of the same breeds, the difference amounting on the average to 4 or 5 respirations per minute. There appears to be no steady constant difference between chronic alcoholists and normals in body temperature.

b. The tanked control birds show no significant change in temperature or respiration as a result of an hour's sojourn in an empty tank.

c. On the contrary the birds which stay one hour daily in a tank containing alcohol vapor exhibit marked and definite changes in their physiological condition at the end of the hour. These changes consist of a *drop in temperature*, amounting on the average to nearly $1\frac{1}{4}$°F. in the case of methyl treatment, and about half as much in the case of ethyl, and a coincident *increase in respiration rate*, amounting on the average to about ten respirations per minute, or a 50% increase in the ethyl, and to about four and one-half respirations, or a 25% increase in the methyl.

d. These immediate physiological changes are precisely the same in kind and comparable in amount to those which have been shown by Völtz and Baudrexel[7] to follow the taking of moderate to large doses of ethyl alcohol by drinking, in the case of dogs and men.

5. I have interpreted the results on parental alcoholism which have been obtained by myself and other workers as due to a selective action of the agent upon the germ cells, in the sense of killing, or at any rate

inactivating, the weak germ cells and permitting only the strong, vigorous, and resistant germ cells to form zygotes. This results, in forms like the fowl having germ cells rather highly resistant to alcohol, in a small but superior progeny from alcoholists. On the other hand, in forms like the guinea-pig, whose germ cells are known to have a relatively low resistance to alcohol[8] it results in the formation not only of a relatively small progeny, but also a weak and abnormal one, since the germ cells which are not entirely inactivated by the agent are still not sufficiently resistant to be uninjured by it.

A rather striking confirmation of this interpretation was obtained in an experiment carried out this past summer. I reasoned that if the above interpretation is correct, alcohol ought to act as a selective agent in the same sense on the very young zygotes, killing the weak and permitting the survival only of the strong provided the appropriate dosage could be found. To test this idea the following experiment was performed. Two incubators were used, each containing at the outstart 390 eggs. These eggs were selected with the utmost care to ensure likeness of age, of strain, and of other characteristics in the two lots. In one incubator 40 cc. of 95% ethyl alcohol were evaporated beneath the eggs daily. In the other incubator no alcohol was used. At the end of 7 days 130 eggs, designated as Lot 1, were removed from the alcohol incubator and allowed to complete their development in a normal non-alcoholic incubator. At the end of fourteen days the remaining eggs (after testing out infertiles) of Lot 2, which originally contained 130 eggs, were removed from the alcohol incubator and finished their development without further dosage of alcohol in a normal incubator. Finally Lot 3, originally containing 130 eggs, was subjected throughout the 21 days of incubation to the daily dosage of alcohol fumes. At hatching all the normal chicks from both incubators were put together in the same house and brooder and given throughout life the same treatment as to feed, etc. Careful account was kept of mortality, each chick, being marked so that it could be told from which lot it came. The conditions of brooding were purposely made bad so to obtain a maximum severity of post-embryonic environmental conditions with a consequent high absolute mortality rate. The results are set forth in Table 4.

TABLE 4

DATA ON HATCHING AND MORTALITY OF CHICKS FROM ALCOHOL TREATED EGGS

Eggs set May 15. Hatched June 5

CHARACTER	LOT 1	LOT 2	LOT 3	CONTROLS
	Alcohol 1 week	Alcohol 2 weeks	Alcohol 3 weeks	No alcohol
Eggs set	130	130	130*	390
Eggs infertile	34	35	41	111
% infertile	*26.2*	*26.9*	*32.3*	*28.5*
All prenatal deaths	41	41	52	108
% of fertile eggs	*42.7*	*43.2*	*60.5*	*38.7*
Number good chicks in brooder	53	52	32	138
Dead in first 30 days	15	11	12	47
% chicks in brooder	*28.3*	*21.2*	*37.5*	*34.1*

* Three eggs were broken during incubation from this lot.

From this table we note the following points:

a. The original sorting of the eggs in the different lots was very fair and equal as is indicated by the nearly equal percentages of infertility.

b. The prenatal mortality was *higher* in all treated lots than in the controls, the differences ranging from 4% to 21.8%.

c. The postnatal mortality was *lower* in Lots 1 and 2, where the eggs had been alcohol treated for one and two weeks of incubation respectively. The decrease in postnatal mortality in Lot 2 amounted to 12.9%.

d. In Lot 3 the dosage was evidently too severe and the chicks which hatched had been injured by the treatment during incubation, with the result that they showed a somewhat higher postnatal mortality than the controls.

e. Synthesizing the results we may say that the more alcohol the embryos received during incubation the higher was the prenatal mortality, but until the dosage became so prolonged as to injure *all* the zygotes, as in Lot 3, the prenatal mortality was *selective*, since the higher the prenatal death rate the lower was the postnatal mortality among the hatched chicks.

f. The general result set forth in the preceding paragraph is contrary to the usual experience of poultrymen with ordinary untreated eggs, which is that if a lot of eggs hatch badly, many dying during incubation, a relatively high postnatal mortality among the chicks is also to be expected. It is believed that the present results may have an important practical application in the poultry industry.

In summary it may be said that the results during the 1916 breeding season confirm and extend the earlier results regarding the effects

of parental alcoholism on the progeny in poultry. The additional data make still more certain the conclusions, first, that the progeny of alcoholized parentage (in poultry) while fewer in numbers is made up of individuals superior in physiological vigor, and, second, that this result is due to a selective action of the alcohol upon the germ cells.

[1] Papers from the Biological Laboratory of the Maine Agricultural Experiment Station, No. 105.

[2] Pearl, R., *Proc. Nat. Acad. Sci.*, **2**, 380–384 (1916). *Cf.* also for a fuller statement of the results *Proc. Amer. Phil. Soc.*, **55**, 243–258 (1916). The complete report on the earlier stages of the work is in press in *J. Exper. Zool.*

[3] This term is defined in my former papers as "the total number of days during which the two gametes making the offspring zygote have been exposed to alcoholic influence while sojourning in the body of the treated individuals."

[4] For summary and bibliography of earlier papers see Stockard, C. R., and Papanicolaou, G., A further analysis of the heredity transmission of degeneracy and deformities by the descendants of alcoholized mammals, *Amer. Nat.*, **50**, 65–88, 144–177 (1916).

[5] Tyson, H. H., and Schoenberg, M. J., Experimental researches in methyl alcohol inhalation, *J. Amer. Med. Assoc.*, **63**, 915–922 (1914).

[6] Poincaré, L., Sur les dangers de l'emploi de l'alcool methylique dans l'industrie, *Paris, C. R. Acad. Sci.*, **87**, 682–683 (1878).

[7] Cf. for example Völtz, W., and Baudrexel, A., Ueber die vom tierischen Organismus unter verschiedenen Bedingungen ausgeschiedenen Alkoholmengen. II. Mitth., *Pflügers Arch.*, **142**, 47–88 (1911); and other papers in the same series. Also note similar data in Völtz, W., Förster, R., and Baudrexel, A., Ueber die Verwertung des Bierextraktes und des Bieres im menschlichen und tierischen Organismus. *Ibid.*, **134**, 133–258 (1910).

[8] Cf. Ivanov, J., Action de l'alcool sur les spermatozoïdes des mammifères (Première communication), *C. R. Soc. Biol., Paris*, **74**, 480–482 (1913).

AN IONIZATION MANOMETER

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Heretofore the only manometers available for measuring extreme vacua have been the Knudsen manometer and the Langmuir molecular gauge. Both of these have serious disadvantages due to their delicate construction and slowness of action. A new manometer free from these objections and with a greater range of presssure than either has been developed. This manometer makes use of the ionization of gas by an electron discharge.

The manometer consists of three electrodes sealed in a glass bulb which serve as cathode, anode, and collector of positive ions. The cathode may be any source of pure electron discharge such as a Wehnelt cathode or a heated tungsten or other metallic filament. The exact forms of the electrodes are not of great importance. The collector is

preferably situated between the other two electrodes and of such form as not to entirely block the electron current to the anode. A milliammeter is used to measure the current to the anode and a sensitive galvanometer to measure the current from the collector which is maintained negative with respect to the cathode so as to pick up only the positive ions.

If there were no gas at all in the space between the electrodes a pure electron current would flow from cathode to anode and no current would flow to the collector. However, if gas is present positive ions are formed by collision in amount proportional to the electron current and the number of gas molecules in the space. Since the collector is negative with respect to the cathode a certain proportion of the positive ions, depending on the form, dimensions, and potentials of the electrodes, will flow to the collector. Hence the ratio of the collector current to the anode current is proportional to the pressure and may be used to measure the pressure when the constant of proportionality has been determined.

This relation has been tested experimentally with air over a pressure range from 10^{-3} to 4×10^{-6} mm. of mercury by comparison with McLeod and Knudsen manometers. The actual apparatus used consisted of a glass bulb 6 cm. in diameter enclosing three parallel, V shaped filaments of thin platinum strip, each about 3.5 cm. long, placed 5 mm. apart, the collector being between the other two. Leads from both ends of each filament were brought through the glass. This arrangement permits glowing the electrodes to free them from occluded gases. An oxide coated filament was used for the cathode. The bulb was sealed to a large glass reservoir which was connected to a high vacuum pump and either the Knudsen or McLeod manometers. When the latter was used a liquid-air trap served to keep the mercury vapor of the McLeod manometer out of the ionization manometer.

Currents from 0.2 to 2.0 milliamperes were used with from 100 to 250 volts between cathode and anode. The collector was held at 10 volts negative with respect to the cathode. The resulting current to the collector at a pressure of 10^{-3} mm. was about one-thousandth the current to the anode and at lower pressures was proportionately less. Hence at a pressure of 10^{-6} mm. with a current of 2.0 milliamperes to the anode a collector current of 2×10^{-9} amperes could be obtained. With a sensitive galvanometer much lower pressures could easily be measured.

Experiments with hydrogen and with mercury vapor in place of air gave constants of proportionality nearly the same as with air.

The advantages of this type of manometer are readily apparent. Its range compared to that of other high vacuum gauges is very large, extending from more than 10^{-3} mm. to as low pressures as can be obtained, without any change of apparatus. On account of its simplicity of construction it is inexpensive and exactly reproducible. Since there are no moving parts there are no difficulties due to vibration. The pressures of vapors which would not be registered on the McLeod gauge are measured by the ionization manometer. One of the greatest advantages is the rapidity and ease with which measurements of a varying pressure may be made since only the reading of a galvanometer need be followed.

Many applications for which other manometers cannot readily be used at once suggest themselves, such as the measurement of vapor pressures of metals, etc. Since the device may be made with extremely small volume the pressure of very small quantities of gas may be measured. It would also be useful to measure pressure changes over a long period of time for which more expensive manometers could not well be employed.

A number of interesting physical measurements other than the measurement of pressure can be made with devices operating on the principle of this manometer, among which is that of the removal of occluded gases by electron bombardment. It is also hoped that experiments with various gases will give some information as to the relative cross sectional areas which different kinds of molecules present to the electron discharge, for although the constant of the manometer was found approximately the same for hydrogen, air, and mercury vapor, more exact measurements might show differences due to different molecular diameters.

PHYSIOLOGICAL STUDIES ON RHIZOPHORA

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Research on the physiology and ecology of the red mangrove, *Rhizophora mangle*, which has engaged my attention for the past few years, was continued at the Tortugas Laboratory of the Carnegie Institution during the summer of 1916. The phase of physiologic investigation most emphasized was that relating to the transpiration rate of *Rhizophora* seedlings grown in solutions of different concentrations of salt

water and in various soils. The mangrove, growing as it does in such peculiar conditions, in salt and fresh water alike, offers a rich field for studies on absorption and transpiration rates, the effects of chemical substances on these phenomena, and the physical relations of density of media. A brief account is here given of some of the more important observations made on the effects of these changes in media on the transpiration.

The material used was *Rhizophora* seedlings. These were selected as the most easily handled and the most readily procurable. Seedlings one to two years old, growing on shores of keys from 3 to 75 miles from the Tortugas, were secured and planted, some in Tortugas sand (a coarse sand composed of broken shell, corals, and other calcareous débris) and others in a ferruginous sandstone soil brought down from New Jersey. Another series also was planted in fine calcareous mud taken from the moat at Fort Jefferson. These cultures compose the soil series of experiments.

Another group of plants was planted in the Tortugas sand and kept in water of the following concentrations: 100, 75, 50, 20, 10, and 5% fresh, and 100% salt water. In a previous season records were taken on plants grown in hyperconcentrated sea-water, 140% salt. The record for these plants gave a very slow rate of growth, and after lingering for a few weeks, the plants yellowed, dropped the leaves, and died. These experiments were not repeated.

In all these cultures the plants were grown in large 10-inch beakers and the water was siphoned off every day and a fresh solution was put on. This was found to be necessary to keep algæ and mosquito larvæ out of the cultures and also to simulate the tidal action in daily bringing a fresh supply of water to the plants, as in their natural habitat.

Still another group of cultures was made of plants kept in various light intensities as well as moisture conditions. One class was kept in New Jersey soil in partial shade with 100% salt water; another in partial shade, New Jersey soil, 50% salt water; a third class, kept in partial shade in shell sand, merely moistened with 100% salt water; a fourth class was kept in partial shade in New Jersey soil, merely moistened with 100% salt; and a fifth class in full sun all day long, planted in shell sand and kept moist with 100% salt water.

This last class was in a condition which most nearly approximates the natural environment of *Rhizophora* seedlings cast up by the waves on a coral strand in the Gulf region. The tides bury the hypocotyl or it bores a resting-place by the radicular end twirling in the water-current and often becomes entirely covered with sand. Leaves are put

out as long as the source of moisture in the sand is constant and there is enough reserve food in the hypocotyl, but the tiny leaves are soon burned up by the fierce sunlight and the drying winds. This putting out of leaves happens repeatedly until the little seedling is exhausted and it succumbs to the hard conditions on the beach. The same thing occurred in the full-sun cultures, so that no leaves could be secured on which to take transpiration records.

The method of securing the records was that of Stahl—that is, a colorimetric method. A Ganong leaf-clasp was employed and the rate of transpiration was measured in minutes and seconds until the cobalt chloride paper in the clasp was changed to a uniform pink, due to the water given off through the epidermis of the leaf, chiefly that of the lower surface, as there are no stomata on the upper surface. Potometer records were also taken, but on account of the limited amount of cultures this method was not feasible. The plants in the soil and water concentration cultures could have repeated tests taken on them by the Stahl method, whereas for a potometer record a plant would have had to be sacrificed for every reading. The readings were taken mostly during the middle portion of the day, to secure as uniform conditions as possible. Some potometer records were made to check up results and to get some quantitative idea of the amounts of water really transpired.

In tabulating the results of this work it is found that by plotting these transpiration readings (given below in part) with the time intervals as ordinates and the concentration percentages as abscissæ, a parabolic curve is described. The following is a statement of the solution concentrations and transpiration intervals:

Series A, 100 per cent fresh, 1.6 minutes
Series B, 75 per cent fresh, 1.7 minutes
Series C, 50 per cent fresh, 2.4 minutes
Series D, 20 per cent fresh, 2.8 minutes
Series E, 10 per cent fresh, 3.2 minutes
Series F, 5 per cent fresh, 3.9 minutes
Series G, 100 per cent salt, 4.1 minutes

By applying the formula $Y = CX^n$, the constant being the initial quantity of water needed to affect the transpirometer, it is also deduced that the additional time required for the transpiration of equal quantities of water is due to the retarding effect of progressively higher salt concentration of the medium—that is, the time factor is a function of the square of the concentration divided by a constant. In other words, the rate of transpiration varies directly with the concentration of the medium in which the *Rhizophora* plants grow.

For the soils series of experiments the data show that the most striking phenomenon is the accelerating effect on the transpiration due to the soil from Maplewood, New Jersey. It is assumed that this accelerating effect is due to chemical action, the New Jersey soil having a larger number of elements in it, and others in greater amount (viz., iron, aluminium, and silica) than in the shell sand of the Tortugas, in which calcium carbonate preponderates. Though, unfortunately, the range of data is not wide enough on the New Jersey soil to produce a full curve, there is enough to show that the segment paralleled the parabola of the shell sand curve, which curve is expressed with the concentration of the water being constant for both soils.

Another interesting point, illustrated in this latter plotting, particularly of the data secured on the cultures which were kept merely moist with the salt water, was the demonstration of the physiological law that the smaller the quantity of available moisture the slower the transpiration. Of course, this was only to be expected, but the clarity of the evidence presented in this case was a pleasant surprise to the investigator, inasmuch as he was only indirectly concerned with the water available for absorption.

Casual mention may here be made also of a series of biochemical tests conducted on *Rhizophora* hypocotyls this summer, to determine the relations of the amounts of tannic acid and dextrose in these organs; and also to demonstrate, if possible, the presence of the enzyme tannase —broadly, to learn something of the rôle of tannin, which is so abundant in *Rhizophora* in nearly all its tissues. This work, while merely started, presents a very interesting field of investigation, since it is supposed that some tannins of the plastic group probably contribute to the nutrition of the plants containing them. It is hoped to continue work on this problem, since the knowledge of the function of tannins in the plant economy is rather obscure, and generally they are supposed to be merely excretory products. On account of the paucity of data in this research the writer does not feel justified in making a definite statement regarding the relation of the amounts of tannic acid and dextrose in the *Rhizophora* hypocotyl; at present, however, it may be safely said, on the strength of over 50 tests for tannase, conducted according to the usual methods of enzyme tests, that this enzyme could not be detected in the hypocotyl of *Rhizophora*.

ON THE HYDROGEN ION CONCENTRATION OF SEA WATER, AND THE PHYSIOLOGICAL EFFECTS OF THE IONS OF SEA WATER

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Both natural sea water at Tortugas, Florida, of salinity of 35–36 per kilo and artificial sea water of the following composition were studied:

	Normal solutions	or	*Isotonic solutions.*
$CaCl_2$ ..... 0.5 m.	22.07 cc.	0.38 m.	29.0 cc.
$MgCl_2$ ..... 0.5 m.	50.21 cc.	0.37 m.	67.9 cc.
$MgSO_4$ ..... 0.5 m.	57.09 cc.	0.975 m.	29.5 cc.
KCl ..... 1.0 m.	10.23 cc.	0.577 m.	17.7 cc.
NaCl ..... 1.0 m.	483.65 cc.	0.568 m.	852.0 cc.
NaBr ..... 1.0 m.	0.80 cc.	0.565 m.	1.4 cc.
$NaHCO_3$ ..... 1.0 m.	2.40 cc.	0.93 m.	2.58 cc.
H_2O	373.63 cc.		0.00 cc.
	1000.00 cc.		1000.00 cc

The mixture was aerated many hours or until it reached the proper alkalinity. It was tested by placing the most delicate marine organisms in it, which were found to live and behave normally.

The excess of non-volatile base over mineral acid in Tortugas sea water was found by Dole to be 0.002265–0.00253 N. per kilogram, and my own observations fall within these limits. That of various artificial sea waters studied was 0.00225–0.00245 Normal per kilogram. This variation in natural sea water is independent of the salinity and is apparently associated with the variation in the lime content.

The hydrogen ion concentration will be indicated by the minus logarithm or PH, the PH of pure water, or neutral solution, at 23° being 7, that of *n*. HCl ca. 0, and that of *n*. KOH ca. 14. The PH was determined by the hydrogen electrode and standardized instruments, the special apparatus being described by McClendon and Magoon[1] All measurements were made at 30°. The PH of Tortugas sea water was found to vary from 8.1 to 8.22 and this variation was found to be partly due to the variation in the excess base but largely due to variations in the CO_2 tension.

The PH of sea water at known CO_2 tensions was studied and it was found that, as the CO_2 tension decreased, the ratio of change in PH with change in CO_2 tension increased so that the PH could be much more accurately measured than the CO_2 tension. The CO_2 tension of the

sea varied from about 0.03–0.05% of an atmosphere, but these measurements will be made more accurately next summer. One estimation of the CO_2 of the air was 0.033%. It seems probable that CO_2 is passing from the sea to the air at Tortugas, but this question also will be studied more accurately next summer. From Fox's data and the above measurements, the following estimations at 30° may be made:

CO_2 content in cc. per litre of sea water	44.5	46.25	47.5
CO_2 tension in percent of an atmosphere	0.03	0.04	0.05
PH	8.22	8.15	8.1

From this table the CO_2 content may be estimated from the PH and hence in respiration may be made by estimating the PH of water containing animals, at certain periods. For this reason rapid methods of estimating the PH were calibrated by comparing vials of Sörensen's borate mixtures colored with thymolsulphonephthalein[2] with a similar vial of sea water to which is added the same quantity of thymolsulphonephthalein, and the PH read off colorimetrically. It was found that when the sea water was tested with the same hydrogen electrode used in standardizing the borate mixtures its PH was found to be 0.3 lower than that estimated colorimetrically, owing to the salt action on the indicator, consequently 0.3 must be subtracted from the result obtained colorimetrically. The salt error could not be determined more accurately with these tubes since they read down only to 0.1 on the PH scale but I understand that Lubs and Clark are studying this question in detail and may have more exact data.

Some general experiments on the physiological significance of the ions of sea water were made, but since they were intended to be general, no extremely exact quantitative data were recorded. Hence these experiments cannot be compared with those of A. G. Mayer on the same subject in which he has made some exact determinations of the effect of H and OH ions at various concentrations. Since I have shown that ions may change the permeability of irritable cells, and excitation seems evidently to be associated with increase in permeability to ions and I have also shown that it is accompanied by increase in electrical conducitivity; the question arises whether all ions may be divided into two classes: those which increase permeability and those which inhibit this change. Experiments on the pulsation of the jelly fish, *Cassiopea*, and the heart of the Conch, *Strombus*, indicate that OH', $Na^{\cdot}$, and $K^{\cdot}$ increase permeability, and that $H^{\cdot}$, $Mg^{\cdot\cdot}$, and $Ca^{\cdot\cdot}$ inhibit increase in permeability. Since a certain increase in permeability puts the cell in the stimulated condition which is the same as the re-

fractory period, $K^{\cdot}$ may inhibit stimulation by giving the cell a single stimulus which continues and is manifested by the continuance of the refractory period as long as the potassium ion is present. $Ca^{\cdot\cdot}$ on the other hand is very toxic and when too liberally applied causes death accompanied by increase in permeability and death rigor, which may simulate the stimulated state.

OH', $Na^{\cdot}$, and $K^{\cdot}$ lower the threshold for excitation of the jellyfish and conch heart, and $H^{\cdot}$, $Mg^{\cdot\cdot}$ and $C^{\cdot\cdot}$ raise it. When cautiously applied, OH', $Na^{\cdot}$, and $K^{\cdot}$ stop the conch heart in systole, and $H^{\cdot}$, $Mg^{\cdot\cdot}$, and $Ca^{\cdot\cdot}$ stop it in diastole, but the action of $Ca^{\cdot\cdot}$ may seem the reverse when not carefully studied. This seemingly uncertain condition is probably due to the organism being composed of several kinds of irritable elements, each requiring a balance of ions of the two classes. A balance for all these elements cannot be maintained with one pair of antagonistic ions alone.

Since the plasma membrane or cell surface is very probably composed of emulsoids, or hydrophile colloids,[3] the change in permeability must be due to a colloid change. Although suspensoids are easily coagulated or precipitated, emulsoids have too great an affinity for water to be easily separated from it except in a most gradual manner unless they are changed by certain reagents or temperatures so as to approach the character of suspensoids. We should therefore not look toward the processes of aggregation and dispersion as seen in suspensoids for an explanation of change in permeability. The changes in an emulsoid membrane are to be regarded as swelling and shrinkage as seen in gelatine or collodion plates. Water soluble substances diffuse easily through collodion gels until the shrinkage reaches a certain point, beyond which the permeability rapidly decreases with shrinkage and the collodion finally becomes impermeable to electrolytes. This is probably due to the approximation of the collodion molecules until the pores are too small to admit of free diffusion of hydrated ions through them. It is to be distinguished from mere approximation of colloid particles, since we may have diffusion through a continuous sheet that has swelled in water, and the diffusion probably takes place in the bound or hydrate water itself. Bartell and Bigelow have shown that porcelain becomes rapidly semipermeable when the pores decrease below 0.1 micron.

It was shown by H. N. Morse that KCl and NaCl increase the permeability of colloidal copper ferrocyanide membranes, but it is a generally known fact that Ca and Mg salts do not increase the permeability, at least not sufficiently for their own diffusion or of that of sugar. Na and K salts (especially chlorides) increase the swelling and permeability

of gelatine plates and Mg and Ca salts, at least under certain conditions, retard the swelling and may even cause shrinkage. OH' increases swelling and $H^{\cdot}$ decreases swelling if the reaction of the medium is on the alkaline side of the isoelectric point for the colloid, which seems to be the case in regard to blood and animal proteins within the body or in sea water.

We may therefore conclude that OH', $Na^{\cdot}$, and $K^{\cdot}$ increase the permeability of the plasma membrane by causing it to swell and that $Ca^{\cdot\cdot}$, $Mg^{\cdot\cdot}$, and $H^{\cdot}$ (at least on the alkaline side of the isoelectric point) inhibit increase in permeability by inhibiting swelling.

[1] *J. Biol. Chem.*, 25, 669 (1916).

[2] These vials were obtained from Hynson, Westcott and Company, Baltimore, Maryland.

[3] There is reason to support the view that the cell surface is composed of emulsoids rather than suspensoids.

SOME INTERRELATIONS BETWEEN DIET, GROWTH, AND THE CHEMICAL COMPOSITION OF THE BODY

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Changes which normally occur in the water, ether extract, and ash content of the body during its most active growth have been determined for the white mouse. Based on eighty-eight analyses of the entire body at different stages of growth, the following results were obtained: (*a*) increase in solids from 16% at birth to a maximum of 35% at fifty days with a subsequent decrease to 33%; (*b*) decrease in the proportion of water in the fat-free substance from 85.5% at birth to 73% in the adult mouse; (*c*) rapid increase in fat during the first twelve days from 1.85% to about 10%, followed by a slow increase to about 12%; (*d*) absolute and relative increase in ash content from 25 mg., 1.86%, in a mouse weighing one and a half grams at birth, to 950 mg., 3%, of ash in the adult mouse.

The ash content of mice growing normally on an artificial food mixture of milk powder, casein, starch, salts, and butter fat, (protein 30%, fat 32%, and ash 5.5%), is uniformly higher at corresponding stages of growth than the ash content of mice fed on a ration of mixed grain, dog bread, and small amounts of milk. It is not known to what this difference is due, but it is not related to differences in the amount of protein or salts in the food. Since the ash content of normal animals may be thus increased by differences in diet alone, it would seem that

conclusions as to the development of the skeleton during arrested growth which are based on an increased ash content of the body, should depend upon comparison not with the 'normal animal,' but with the normal animal on the same diet.

When abundance of fat is furnished in the diet, but not enough protein to maintain normal growth, the fat content of the animal is greater than when the food contains an adequate amount of protein with the same proportion of fat. There seems to be a tendency to protect the limited amount of protein by increasing the available supply of fat in the body. This does not occur when growth is arrested by lack of lysine, as in the use of gliadin as the only protein in the diet, since in this case the limiting factor lies not in the amount but in the nature of the protein.

By underfeeding, mice have been completely arrested in growth, as far as growth is expressed in gain in weight, and have been maintained at a constant body weight of 12 grams for thirty and sixty days. At the end of the thirty-day period, control mice of the same initial weight as the experimental animals, and on the same diet, weigh 22 grams. Comparison of the composition of the stunted mice with that of mice growing normally shows that the proportion of fat in the stunted animal is about the same as in the normal mouse of the same weight (but younger), while the percentage of water in the fat-free substance corresponds to the water content of a normal mouse of the same age (but heavier). That there is no evidence of a general replacement of fat by water, such as is often reported in underfed animals, may be due to the large proportion of fat used in this diet. The ash content, both absolute and relative, of the stunted mouse is greater than that of the normal mouse of the same weight, confirming in this the results reported by Aron for the rat, and indicating continued growth of the skeleton under conditions which prevent the animal from gaining in weight. The tendency of the skeleton to develop under such adverse conditions does not appear to be as strong in the mouse, however, as in the rat, if the few data reported for the rat are representative.

Retardation of growth by other means—reduction of protein or of salts in the food, or substitution of gliadin for other proteins—affects the ash content of the animal in the same way as simple underfeeding.

If young mice which have been maintained for a time at constant weight are given sufficient food again, they grow at a greatly accelerated rate which enables them to overtake control mice which have grown uninterruptedly. The ash content, however, does not increase at the same rate as the body weight; and the development of the skele-

ton, as represented by changes in the total ash of the body, which proceeded for a time at the expense of other tissues while the animal was held at constant weight, does not now, in the active growth which accompanies refeeding, keep pace with this rapid gain in body weight, and consequently in a few days the normal relation is almost reestablished.

FURTHER STUDY OF THE ATOMIC WEIGHT OF LEAD OF RADIOACTIVE ORIGIN

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The recent independent and almost simultaneous investigations upon the atomic weight of lead from radioactive minerals have proved with very little room for doubt that the substance derived from this source has a much lower atomic weight than ordinary lead.[1] This conclusion is so important in its theoretical relations that its every aspect should be carefully investigated. Accordingly, the present paper represents further research in this direction, embodying determinations of the atomic weight of new samples of varied origin. The outcome entirely supports the earlier conclusion.

Four samples from widely separated sources were studied in the present research, namely, lead from Australian carnotite, from American carnotite, from Norwegian cleveite, and Norwegian bröggerite.

The first of these samples was obtained in large quantity through the kindness of Mr. S. Radcliff and Mr. E. R. Bubb, of New South Wales.

The preliminary purification of the sample was carried out in Australia.[2] Our subsequent purification was briefly described in a recent paper,[3] but some additions to the account are needed. The metallic lead was dissolved in nitric acid, leaving practically no residue. A portion of the nitrate thus obtained was precipitated with 20% hydrochloric acid from dilute solution. Lead sulphide was precipitated from the warm acidified solution of this chloride by pure hydrogen sulphide, and after separation and washing was dissolved in pure nitric acid. The small portion oxidized to sulphate during this process was boiled with sodium carbonate and the lead carbonate, washed free from sodium, was dissolved in nitric acid and united to the main portion of the nitrate, which was recrystallized four times from pure water and precipitated as chloride from a warm solution in a quartz

dish with hydrogen chloride. The precipitate was centrifuged, dissolved in water and recrystallized four times, each time having added a few drops of hydrochloric acid to prevent the formation of basic salt. The second mother liquid gave no test for nitrate. This chloride formed Sample A, and after two additional recrystallizations a similar specimen constituted Sample B.

The second portion of the original nitrate made from the Australian sample was purified in a much simpler fashion. Avoiding the troublesome precipitation with hydrogen sulphide, the nitrate was recrystallized five times successively by adding concentrated nitric acid to its aqueous solution; then the lead was converted into chloride, and by precipitation with excess of hydrochloric acid this salt also was recrystallized five times. This was Sample C of the chloride, a portion of which before the last crystallization had served to prepare Sample D of the metallic isotopic lead used for determining the density.[4]

The next sample, designated F, was prepared from American carnotite and came to us in a considerably purified state through the kindness of Dr. C. H. Viol of Pittsburgh, of this company, and of Prof. W. D. Harkins. The method of further purification was essentially like that just described.[5]

Two other samples of especial value and significance were obtained through the kindness of Dr. Ellen Gleditsch, of Kristiania. Both came from primary rocks—Norwegian pegmatite dykes. The purification of one of these, from cleveite, has already been described;[6] the source of this material, which occurred in cubic crystals and was carefully selected, was near Langesund, Norway. It was recrystallized first as nitrate and then as chloride three times, each in the usual manner, and the pure substance was designated as Sample G.

Yet another sample, designated H, was prepared from lead sulphide, also kindly sent by Dr. Gleditsch, obtained from selected crystals of Norwegian octahedral bröggerite from Roade, near Moss, Norway. This was purified in precisely the same way as lead from cleveite.

In addition to these four samples containing isotopic lead enough ordinary lead was carefully purified to serve as the basis of control analyses. The purest "test lead" of commerce, free copper, was dissolved in nitric acid and recrystallized four times as nitrate and four times as chloride.

Throughout this work the usual care taken in atomic weight investigations was not forgotten. The nitric acid, hydrochloric acid, water and silver were all purified in methods already often described, and throughout the work on the nitrate and chloride of lead, except

in the preparation of Sample A, the material was treated exclusively in vessels of platinum or quartz. All the weighings were reduced to the vacuum standard and all other precautions usual in this sort of work were carefully maintained.

The analysis was essentially similar in every way to the method described so often in Harvard contributions. The lead chloride was fused in a platinum boat in pure hydrochloric acid; this gas was displaced with nitrogen while the substance was cooling; and finally the pure dry salt in its boat was pushed into the weighing bottle, stoppered in pure dry air with the help of the familiar Harvard "bottling apparatus" and weighed at leisure. The weighed salt was placed in a large Erlenmeyer flask with glass stopper very carefully ground. Enough water was then added to form a fiftieth normal solution of the salt and the flask and contents, with the addition of a drop of pure nitric acid to prevent the formation of basic salt, were gently warmed on an electric stove, at about 50°C., until complete solution was obtained. The boat was then removed, and the residue filtered off, both boat and residue being carefully washed and the filtrate being collected directly in the precipitating flask.

The chlorine contained in this solution was then precipitated in the usual fashion by an amount of silver calculated from preliminary trials to correspond with it as nearly as possible. Any slight deficiency or excess was corrected by adding silver nitrate or chloride, and testing in the nephelometer; and the finally corrected weight is given in the table. The precipitation was carried on in a dark room, under red light, and all the usual precautions were taken.

The first two determinations of ordinary lead were only preliminary, in order to gain practice with the method, and are not included in the table below. They yielded values for the atomic weight respectively 207.15 and 207.16. All the other analyses which were brought forward to conclusion are recorded in the tables, which are self-explanatory.

The atomic weight of ordinary lead

SAMPLE	CORRECTED WEIGHT $PbCl_2$	CORRECTED WEIGHT Ag ADDED	RATIO $PbCl_2$: Ag.	ATOMIC WEIGHT Pb
3	3.72918	2.89325	1.28892	207.179
4	5.35111	4.15151	1.28896	207.188
			Average	207.183

The atomic weight of "isotopic" lead

	SAMPLE	CORRECTED WEIGHT $PbCl_2$	CORRECTED WEIGHT Ag ADDED	RATIO $PbCl_2 : Ag$	ATOMIC WEIGHT Pb
1 A	Carnotite, Australia.....	4.64010	3.61118	1.28493	206.318
2 B	Carnotite, Australia.....	5.35517	4.16711	1.28512	206.359
3 B	Carnotite, Australia.....	6.15608	4.79072	1.28500	206.334
4 C	Carnotite, Australia.....	4.14770	3.22748	1.28512	206.359
				Average	206.342
5 F	Carnotite, U. S. A.....	5.31585	4.12670	1.28816	207.015
6 F	Carnotite, U. S. A.....	4.65899	3.61707	1.28806	206.994
				Average	207.004
7 H	Bröggerite, Norway.....	4.29104	3.34187	1.28402	206.122
8 G	Cleveite, Norway.....	3.92736	3.05913	1.28382	206.079
9 G	Cleveite, Norway.....	4.45270	3.46818	1.28387	206.090
				Average	206.084

The results of these analyses show that the different samples containing isotopic lead all give lower values for the atomic weights than ordinary lead, but that the material from each source gives a different value, precisely as had been previously found in the earlier investigations in this and other laboratories. Ordinary lead gave the maximum value (essentially equal to that found by Baxter[7]) and isotopic lead from Norwegian cleveite gave the minimum value (essentially equal to that found by Hönigschmid in bröggerite[8] 206.06).

It seems reasonable to suppose that the other samples were composed of mixtures of these two kinds of lead. The value 206.34 would be given approximately by a mixture of three parts of isotopic lead like that obtained from Norwegian cleveite with one part of ordinary lead—a reasonable supposition, since the Australian carnotite was known to have contained galena. The American carnotite, Sample F, had an atomic weight which would be given by a mixture of only one part of the pure isotope with 5 or 6 of ordinary lead,—a condition which seems to indicate the admixture of very large amounts of galena with the sample in question.

Two physical properties of the several preparations under consideration have especial interest, namely, the magnitude of the radioactivity and the nature of the spectrum. Both were studied in the present research. We confirmed the outcome of earlier work[1] that the radioactivity is not proportional to the decrease in atomic weight in samples containing isotope coming from different sources. If the radioactivity were dependent upon the presence of the isotope radium

G (the supposed-end product of the decomposition, the present form of isotopic lead), the rate of fall in the gold leaf electroscope for the lead from cleveite would be the greatest, and that from the American carnotite the least; but our results were precisely the other way about. There can be little question, then, that radioactivity is not due to the isotope which gives the low atomic weight. Probably it is due to radium E, since in all of our samples the half-value of the radioactivity was obtained in about five days, the half life of radium E. The maximum value, approached asymtotically, is nearly reached in a month.

The spectrum of isotopic lead as thus far studied by Hönigschmid, by Merton and by Harkins, as well as by Baxter (who kindly photographed the ultraviolet spectrum of one of the samples of lead prepared by one of us with the help of Dr. Lembert) has always been found to be essentially like that of ordinary lead. In the present research we thought it worth while once more to test this question and especially to extend the inquiry to the visible portion of the spectrum—most of the tests in the past having been made in the ultraviolet region. We found difficulty in eliminating traces of copper, silver and calcium too small to be detected by ordinary analysis. Although these were far too diminutive to affect the atomic weight, we continued the purification, and after eight recrystallizations as nitrate and two as chloride, the product gave in every respect precisely the same spectrum as the purest ordinary lead prepared by Baxter and Grover, except for a vanishingly small trace of two of the most prominent silver lines.

The photographs of the ultraviolet region in the Féry spectrometer were very kindly made by Professor Baxter. Photographs of the visible portion of the spectrum were made by us in the Gibbs Laboratory on specially prepared plates sensitive as far as wavelength 7800. All the work of loading and developing had to be done in complete darkness. To make assurance doubly sure, a further study of the visible spectrum was made, in collaboration with Mr. Norris F. Hall, with the help of the Hilger wave-length spectrometer, comparing visually in the same field of view the spectra of pure ordinary lead and the best purified specimen from Australian carnotite. Every line was scrutinized between the range 4000 and 7600, especial pains being taken in the red and yellow portions, the least satisfactory from a photographic point of view. No discernible differences between the two spectra were observed.

Because no lines were detected between wave-lengths 7800 and 2200 in any of the samples which were not due either to ordinary lead or to unimportant traces of well-known impurities, one of the alterna-

tive conclusions previously reached by Richards and Lembert is supported, namely, that this isotopic lead possesses the same spectrum as ordinary lead. The present outcome is especially interesting because the isotopic lead obtained from the preparations of Dr. Gleditsch was probably almost pure—certainly much purer than that examined in 1913.

Since the atomic weight is variable, but the spectrum and atomic volume[9] of these samples all the same, one can hardly avoid concluding that a part of the atom exercising an important effect upon the atomic weight is without influence upon the spectrum or volume. The dual nature thus postulated is, of course, in accord with the interesting hypothetical assumptions which have been advanced by various authors concerning the possible makeup of the atom; but our present research can go not further than support the idea of duality without defining exactly of what the two parts may consist.

We are glad to acknowledge our indebtedness to the Carnegie Institution of Washington for generous support in this investigation.

Summary.—In this paper the atomic weight of four different samples of isotopic lead not hitherto tested, as well as one sample of ordinary lead (used to control the others), was determined. The results were as follows:

Ordinary lead	207.18
Isotopic lead (Carnotite, Colorado)	207.00
Isotopic lead (Carnotite, Australia)	206.34
Isotopic lead (Bröggerite, Norway)	206.12
Isotopic lead (Cleveite, Norway)	206.08

That the most carefully selected sample should give the lowest result is strong (although not absolutely conclusive) evidence that the higher results obtained from other samples were due merely to the accidental admixture of ordinary lead. No new lines were found either in the ultraviolet or visible spectrum of any of these samples. Each, except the ordinary lead, possesses radioactivity, but the magnitude of this radioactivity seemed to bear no relation to the lowering of the atomic weight.

[1] Richards and Lembert, *J. Amer. Chem. Soc.*, **36**, 1329 (1914); Hönigschmid and St. Horovitz, *Paris, C. R. Acad. Sci.*, **158**, 1798 (1914); M. Curie, *Ibid.*, **158**, 1676 (1914); Soddy and Hyman, *London, J. Chem. Soc.*, **105**, 1402 (1914); also especially, Hönigschmid, *Sitz. k. Akad. Wiss., Wien*, IIa (Dec., 1914).

[2] A description of the details is to be found in the paper by S. Radcliff, *J. Proc. R. Soc.*, New South Wales, **47**, 145 (1913).

[3] *J. Amer. Chem. Soc.*, **38**, 223; these PROCEEDINGS, **2**, 505 (1916).

[4] Loc. cit., these PROCEEDINGS, **2**, 505 (1916).

[5] A fuller description is given in our paper published in the December, 1916, number of *J. Amer. Chem. Soc.*

[6] Richards and Wadsworth, *J. Amer. Chem. Soc.*, 38, 1659 (1916).

[7] Baxter and Grover, *J. Amer. Chem. Soc.*, 37, 1027 (1915).

[8] Hönigschmid and St. Horovitz, *Sitz. k. Akad. Wiss., Wien*, 123, IIa, 1 (2407) (Dec., 1914). He gives the name 'Uranblei' to this form of lead. The name is appropriate if, as seems probable, the substance may ultimately be traced back genetically to uranium, according to Boltwood's brilliant and well-supported hypothesis. Because the relation of this form of lead to radium is somewhat less remote than that to uranium, we used the term 'radio-lead' for it in a previous paper; but this term has also been applied to Radium D and is, therefore, not distinctive. The time for a final nomenclature of these substances has probably not yet come, but the expression "isotopic lead," based upon Soddy's word 'isotope,' is certainly safe as applied to every substance fitting into this place in the Periodic System. We venture to suggest that the present permanent isotope of lead be called 'isolead,' because it is now by far the best known of these isotopes. If any other permanent leads are verified, they might be called 'meta' and 'para.' For the highly transitory isotopes these names would be inappropriate, since they do not resemble lead in one of its most characteristic properties, namely, permanence. These might be called 'pseudo-leads,' giving them Greek ordinal prefixes to distinguish between them. But we offer these suggestions without any desire to be insistent, and have not even adopted this nomenclature in the present paper. It may well be best to retain the present nomenclature, especially as regards the transitory isotopes.

[9] See Richards and Wadsworth, these PROCEEDINGS, 2, 505 (1916).

ON SOME ANOMALIES IN GEOGRAPHIC DISTRIBUTION OF PACIFIC COAST MOLLUSCA

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The islands, usually called the Santa Barbara Islands, lie off the coast of southern California from 15 to 50 miles roughly parallel to the shore of the mainland, from which they are separated by a depth of about 400 fathoms. They range in latitude from 33° to 34°N. That they were formerly connected with the mainland seems probable from general considerations, as well as the fact that the fossil tooth of an elephant is reported to have been found on Santa Catalina.

As we go south we find other islands or banks farther off shore and separated from the mainland by an increasing depth averaging more than 1500 fathoms. The Cortez Bank lies about 120 geographical miles south of Santa Cruz Island of the Santa Barbara group, in latitude 32° 20′N. It is 80 miles west of the mainland and separated by a depth of 1090 fathoms.

Next comes Guadelupe Island in latitude 29° N., 200 miles south of the Santa Barbara islands and 150 westward from the nearest Lower California mainland, from which it is separated by a depth of 1500 fathoms.

Six hundred miles south of Guadelupe is Socorro Island and its neighbors, forming the group of the Revillagigedo Islands. It is 330 miles west of the Mexican mainland in latitude 18° 50′ N., separated by a depth of about 1500 fathoms.

In a general way the sea near the coast of the mainland, west of the edge of the continental shelf, is considerably deeper than it is a relatively short distance seaward; that is to say a sort of trough in the sea-bottom lies near and roughly parallel with the mainshore, for a distance of nearly 1000 miles. The soundings are not numerous enough to warrant a more definite statement, but are sufficient to indicate the above generalization.

Investigations into the distribution of the Molluscan faunas of the Pacific coast of the Americas have been carried on for many years by the writer and are gradually reaching a point where positive conclusions can be drawn. We find, for instance, the Panamic or tropical fauna extending from Point Aguja on the Peruvian coast to Cape St. Lucas and the Gulf of California, and on the western shore of the peninsula of Lower California to Cerros Island, and thence (gradually diluted by the Californian fauna) as far as Point Conception, which however is reached by extremely few of the really tropical species.

There is a certain number of littoral mollusks which live only in shallow water near the tidal boundaries and hence are more subject to the influences of climate than those species which inhabit deeper water. Their limits of distribution are strongly marked.

Now it is a fact that many of these littoral species belonging to the Northern or Oregonian fauna actually reach the island of Guadelupe though stopping far short of it on the coast of the mainland.

More surprising still, we find a number of these northern species living on the island of Socorro, where the normal fauna should be wholly tropical: while on Clarion Island in nearly the same latitude and 220 miles west of Socorro there is Indo-Pacific tropical fauna with conspicuous species like *Voluta deshayesii*.

Socorro has, as far as we know, a strictly American tropical fauna with little Indo-Pacific adulteration, though it is 110 miles nearer to Clarion Island than to the American mainland. The other islands of the Revillagigedo group have not been visited by a collector.

Work has not gone far enough to enable a complete list of the northern species now living on Socorro Island, Latitude 18° 20′ N., to be made, but the following species are included in it.

Mytilus californianus Conrad. Southernmost reported locality on mainland in latitude 24° 40′ N., Magdalena Bay, Lower California.

Pachydesma stultorum Mawe. Southernmost mainland locality latitude 21° 36′ N., San Blas, Mexico.

Protothaca staminea Conrad. Southernmost mainland locality latitude 24° 40′ N., Magdalena Bay, Lower California.

Pholadidea sagitta Stearns. Southernmost mainland locality latitude 32° 24′ N., San Diego Bay, California.

There are also several gastropods, but on none of the islands cited has an exhaustive collection been made, so that a complete list is still a desideratum.

Mr. W. H. Ochsner, naturalist of the Galapagos Expedition of the California Academy of Sciences was the first to discover and collect from fossiliferous sedimentary strata on those islands.

In the case of the recent fauna of Socorro Island, we have at least four characteristically Californian species surviving on the island from 200 to 844 geographical miles south of their southernmost known extension on the mainland.

As the distances are calculated only from the difference in latitude and in geographical miles, while the coast line trends in a southeasterly direction, the actual distances are very considerably larger.

What explanation can be offered of these remarkable anomalies? The facts here presented, with others not yet fully worked out, have led to the framing of the following hypothesis.

Suppose the islands and banks here briefly referred to are the remnants of a peninsula or chain of islands once existing, parallel to the present peninsula of Lower California and connected to the mainland of southern California. During the existence of this peninsula, the cold current down the coast which now gives rise to fogs as far south as Cerros Island, in Pleistocene time, especially during the Glacial Epoch, might have carried its normal fauna far to the southward; this peninsula at the same time protecting from the cold current the waters, normally warm, of the sea between it and Lower California as well as of the Gulf of California, so that the tropical fauna might creep up to the extreme northern limit of these enclosed waters.

With the subsidence of the water barrier most of the northern fauna would have perished, but on the undrowned peaks a few of the more adaptable of the northern forms might have been able to persist or have left their traces in the subsequently slightly elevated Pleistocene sediments.

Our present knowledge of the seabottom west of the inshore deep trough previously referred to, is insufficient to add support to this hypothesis, but the stupendous changes of elevation on the California

coast in very late geological time (see *Proceedings U. S. National Museum*, 1, 3, 1878) add a certain plausibility to it, taken in connection with the facts above recorded of the geographical distribution of the shore mollusca.

It should be noted that the Pleistocene fossils collected on the Galapagos Islands by Mr. Ochsner contain no traces of the northern fauna, though plainly of American derivation and with hardly any intrusion of Indo-Pacific forms. Also that the northern species mentioned appear in the Pleistocene and even older beds of the California coast from San Pedro to San Diego.

A somewhat analogous overlapping of faunas occurs on the southeastern coast of Alaska where a considerable number of boreal mollusks, as well as one of the Arctic hair seals, extend southward in the cold waters of the inland passages of the Alexander archipelago, following the low temperature of the water induced by the drainage from glaciers and the shadows of the highlands bordering these narrow passages which, with the prevalent fogs, cut off most of the sunlight. On the outer fringe of the archipelago where the more normal sea temperatures occur, numerous representatives of the Californian fauna find their way northward at least as far north as the latitude of Sitka, and very likely to Cross Sound.

SOME PSYCHO-PHYSIOLOGICAL PROCESSES AS AFFECTED BY ALCOHOL

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The difficulty of adequately measuring the effects of small amounts of ethyl alcohol upon the human organism is indicated by the number of investigators who have worked in this field, by the variety of methods that have been employed, and more especially by the contradictions in the results. It is impossible to harmonize the published data because of the many and unknown variables. If two investigators would independently employ the same apparatus, methods, doses, subjects, and general conditions, the two sets of results would be comparable and seemingly of unique importance. It is the object of this paper to report a comparison of measurements made under such conditions.

Subsequent to the experimentation which is the basis of the extended report by Dodge and Benedict[1] arrangements were made with one of their normal group (Number VI) for a second series of measurements.

This subject had served during parts of twenty days, in all approximately seventy hours well distributed over a period of six months. The fact that he had showed the smallest positive effect of the alcohol was an especial reason for selecting him for the later tests.

During this second series of six consecutive days (June 29 to July 4, 1914, inclusive) the subject, who was a medical student,[2] was free from the usual duties of the school year. Although serving part of the time as an intern in a nearby hospital, he regarded the period as a vacation and could thus carefully regulate his daily habits. The last food before each experimental session was a hearty meal taken seven hours previously from which coffee and tobacco were excluded. No alcohol was taken during the week other than that given in the laboratory. The experiments began at 8 a.m. and continued until 1 p.m., thus providing a total session of five hours as contrasted with the three hour periods employed by Dodge and Benedict. In the former series the subject was working intensely and an effort was made to guard against any weekly rhythm that might exist on this account by having all the experiments come on the same day of the week. In our experiments there seemed no such logical reason for extending the sessons over a period of weeks; consequently the measurements were made on consecutive days, the alcohol being given on the second, fourth and sixth days. The giving of alcohol doses in this frequency may present the possibility of an additive effect. However, such an effect would apply even more to the normal days and thus theoretically a smaller resulting difference would be shown.

The alcohol for each day was given in a single portion. The dose used was that designated by Dodge and Benedict as 'Dose A,' which contained 30 cc. of absolute alcohol, 7 cc. of orange infusion,[3] 1 cc. of a strong infusion of quassia, a slight amount of saccharine for sweetening, and water to bring the volume to 150 cc. A mixture of the same volume and composition, but without the alcohol, served as the control dose for each of the three normal days. In giving the two mixtures, every effort was made to prevent the subject from distinguishing between the alcohol and control days and he had no way of knowing before the session what the dose for that particular day was to be. As the alcohol frequently produced sensations of warmth in the stomach and flatulency, the subject never failed to identify the mixture before the experiment was over, although he received no confirmatory information regarding his impression. Unfortunately only the 30 cc. dose could be used in the second series. This was in accordance with the expressed desire of the subject who insisted that the 45 cc. dose, which he had taken three

times in the previous series, made him feel uncomfortable for the rest of the day.

For the detailed technique of the various measurements used reference must be made to the careful description by Dodge and Benedict. There were but few minor changes in procedure since it was the intention to repeat the measurements of the first series with the same apparatus and as nearly the same conditions as practicable. In outline the experimental cycle was as follows: (1) Preliminary adjustments; placing of the electrodes for electrocardiograms from body leads; taking of data concerning the subject's general condition. (2) Patellar reflexes stimulated by two pendulum hammers of known weight; separation interval 0.5 seconds, latency and amplitude recorded from the thickening of quadriceps muscle. (3) Sensory threshold for Faradic stimulation measured in β-units, method of Martin. (4) Reaction time in reading isolated 4-letter words these being the same set of 24 words that was previously employed. (5) Finger movements, a modified form of the tapping test recorded photographically. (6) Voluntary tetanus designed to produce rapid fluctuations in the pulse rate. (7) Memory test for series of twelve 4-letter words. The same lists of words were used on each day and in the same order. (8) Eye reaction to peripherally appearing stimuli, photographically recorded. (9) Eye movement speed and accuracy in looking successively at marks separated by 40 degrees. (10) Protective lid reflexes stimulated by two sharp noises, separation interval 0.5 seconds, the photographic record being the shadow of the eyelash movement. (11) Pulse records (electrocardiograms from body leads) each approximately 15 seconds in length. Eleven such records were taken during each experimental cycle.

At the beginning of the second experimental period the subject drank the dose for the day, i.e., either the alcohol mixture or the control mixture. Then followed the patellar reflexes and other measurements in rotation as outlined. These were repeated in the same order in each experimental period. Theoretically, for purposes of comparison, the periods on the experimental days should be equal in number and length, but this ideal of regularity is hard to attain. During the six days, however, there was no accidental interruption which caused the omission of any measurements or enforced a delay of more than ten minutes for the adjustment or repair of apparatus. To complete a period required about one hour. This was approximately twice as long as the periods employed by Dodge and Benedict as in their series all of the measurements were not made on the same day.

In addition to providing for an equal number of normal and alcohol

days, the first period of each day was also normal since the dose was not given until the beginning of the second period. Thus the first period values for each particular day serve to indicate the neural condition of the different processes measured. In the results presented emphasis is placed on the differences between the values obtained for the first period of the day and those obtained for succeeding periods of the same day as indicating the effect produced by the alcohol. To obtain the average difference for any particular day and any particular measurement, therefore, the value for the first period is deducted from the values for the succeeding periods, and the algebraic sum of the individual differences is divided by the number of periods succeeding the taking of alcohol. Such a method, while seemingly indirect, presents the effects of alcohol in a less contaminated form than does the simple comparison of the average values for the different days, as the experimental days are subject to changes in the level of nervous excitability. It cannot, however, be assumed that changes will not occur in the 5-hour period, although they are likely to be smaller than between different days.

With this subject the effects of alcohol do not always retain one direction throughout the experimental day, for a depression during the the first two periods after taking alcohol may change to a facilitation during the last hour or more of the session. To average either the measurement values or the differences between the first and succeeding periods under these conditions would therefore mask important tendencies. The results must be presented in a more analytic form.

A summary of the findings for the different experimental periods following alcohol together with the comparable averages found by Dodge and Benedict both for their normal group and for Subject VI individually are shown in table 1. The values given are the percentile effects of alcohol on the various processes in question, a plus showing increase and a minus decrease, which signs must be interpreted according to the nature of the measurement. Similar processes have been grouped in the table by sections. The first value in Period 2, Section 1, + 3.4 shows that the patellar reflext latency (L′, first reflex) was lengthened 3.4% in the first measurements made after taking alcohol as compared with the condition that existed in the period immediately after the taking of the control mixture. This lengthening of the reflex time was also present in the third and fourth experimental periods (+0.9 and +1.5) but not in so great a degree. In the fifth and sixth periods the sign changes, representing a shortened reflex time.

Since in the later series slightly more than 4 hours experimentation followed the drinking of the dose it is obvious that an average for Periods

2 and 3 is most directly comparable to the former results. A column of such averages is included in the table to aid comparison. This average for the patellar reflex, L′, is +2.1 as compared with +11.1, the average

TABLE 1

THE PERCENTILE EFFECTS OF THE INGESTION OF 30 CC. OF ABSOLUTE ALCOHOL ON A RELATED GROUP OF PROCESSES

PROCESSES MEASURED	EXPERIMENTAL PERIODS FOLLOWING THE TAKING OF ALCOHOL					AVERAGE OF PERIODS 2 AND 3	RESULTS FOUND BY DODGE AND BENEDICT WITH EQUIVALENT DOSE	
	Period 2	Period 3	Period 4	Period 5	Period 6		Average for normal group	Average for Subject VI
Section I								
Patellar reflex								
Latency, L′	+ 3.4	+ 0.9	+ 1.5	− 7.4	− 3.4	+ 2.1	+11.1	+ 0.3
Latency, L″	+ 0.8	+ 4.0	+ 5.8	−15.1	−10.6	+ 2.4	+ 4.0	
Lid-reflex								
Latency, L′	+13.6	+ 5.8	− 2.9	+6.4	+ 4.3	+ 9.7	+ 5.9	− 4.5
Latency, L″	+15.1	+ 1.8	− 7.5	− 3.8	+ 3.8	+ 8.4	+ 6.8	−10.7
Eye reaction time	+15.5	+16.4	+14.7	+11.8	− 1.9	+15.9	− 5.4	− 9.3
Word reaction time	+ 3.2	− 8.2	− 7.4	−10.6	−20.8	− 2.5	− 1.0	−26.0
Faradic sensory threshold	+31.0	+20.4	+18.2	+24.9	− 8.3	+25.7	+21.0	+48.0
Eye movement velocity. Rt. movements	+19.3	+ 6.4	+ 2.1	− 1.4		+12.8	+ 3.4	+25.8
Section II								
Patellar reflex								
Amplitude, A′	− 2.5	+ 3.3	−22.7	−42.0	+ 5.0	+ 0.4	−60.3	+13.0
Amplitude, A″	−24.8	−38.0	−24.0	+62.0	+81.0	−31.4	−38.0	+173.0
Lid-reflex								
Amplitude, A′	−38.0	−33.6	+13.3	−17.7	−16.8	−35.8	−10.7	−18.0
Amplitude, A″	−57.0	−42.8	+25.7	−68.5	+65.8	−49.9	−103.9	−27.0
Progress in memorizing	+10.9	+ 2.5	− 5.9	+27.8	+57.0	+ 6.7	+ 2.5	+13.3
Number of finger movements	− 3.9	− 4.0	− 3.1	− 3.5	+ 7.1	− 3.9	− 8.9	−15.2
Section III								
Pulse Cycle Length								
Immediately before exercise	−11.6	− 3.9	−14.5	− 5.8		− 7.7	− 3.1	− 4.0
During severe exercise	+ 1.3	− 4.0	− 4.0	− 2.7		− 1.3	− 4.1	+ 6.0
Immediately after exercise	− 2.5	− 8.7	+ 2.5	− 1.2		− 5.6		
Average of other pulse data	−12.4	−12.2	−14.8	−12.9	−19.4	−12.3	− 3.0	− 3.5

for the Dodge and Benedict normal group. The earlier result for Subject VI alone is +0.3, which although not large shows him to be in line with the normal group and also with his later percentages for periods 2, 3 and 4.

Referring to one of the measurements placed in Section II of the table, —for example, the number of finger movements performed in 8 seconds,— we find that the percentages given under Periods 2 and 3 are −3.9, and −4.0 (average −3.9), showing a decrease in the number of finger oscillations on alcohol days as compared with the normal days. This decrease in approximately the same degree is found in Periods 4 and 5, while in 6 there is a change in sign, thus representing an increase in the number of finger oscillations. In comparison Dodge and Benedict find −8.9% and −15.2% the averages for the normal group and for Subject VI respectively. These values indicate that Subject VI in both series of experiments conforms to the group.

The percentile changes in the length of the pulse cycle are nearly all minus, indicating a decrease in the length of the individual pulse cycle which of course is equivalent to an increase in pulse rate per minute. The effect of Alcohol in producing a faster pulse is more pronounced with Subject VI than for the average of the normal subjects.

In general the various periods may be compared on the basis of the preponderance of plus or minus signs. In Section I, Period 2, all of the values are plus and range from 0.8 to 31.0, with a central tendency above 12%. In Section II the signs for Period 2 are with one exception minus and show a central tendency of about 19%. It should be borne in mind that plus in Section I and minus in Section II have the common meaning of inferior performance or depression. Period 3 is clearly more like 2 than it is like 4, while 2 and 3 averaged may be compared logically with the percentages for the normal group. The signs agree here almost exactly, measurement for measurement, although the percentages are naturally different. In the one clear case of disagreement, that is, eye reaction time, −5.4% for the normal group as against +15.9% for Subject VI in the second series of experiments, and −9.3% in the first series, it should be stated that Dodge and Benedict for this measurement found a decided practice effect for their subjects which masks the influence of the 30 cc. dose of alcohol. In the first series Subject VI is in harmony with the group. In the second series no practice effect is shown and the reaction time is lengthened. The plus and minus signs in the averages for Subject VI otherwise agree for the most part with his averages for Periods 2 and 3. Detailed discussion of the individual measurements cannot be given here but will appear in a fuller report soon to be published by the Carnegie Institution of Washington.

The two series of measurements for Subject VI taken together unmistakably indicate as a result of a dose of 30 cc. of absolute alcohol a lengthened reflex latency with a decrease in the amplitude of movement,

slower reactions, slower coördinated movements, less sensitiveness to stimulation and an increase in pulse rate. The memory and word reactions, as in the earlier results, were improved after the alcohol.

Some attention should be given to the fact that in Period 6, and to a less extent in Period 5, in Sections I and II of the table, the signs are in the majority of cases the opposite of those in Periods 2 and 3 for similar measurements. This means a superior performance and is in contrast to the earlier condition of general depression. There are some indications in the data previously published that this facilitation following the alcohol depression may not be a peculiarity of Subject VI, but a characteristic phenomenon of the alcohol effect.

[1] Dodge and Benedict, Psychological effects of alcohol, *Carnegie Inst. Washington, Pub.* 232, 1915; these PROCEEDINGS, 1, 605 (1915).

[2] For family and personal history see Dodge and Benedict, op. cit., p. 277.

[3] Rivers, *The influence of alcohol and other drugs on fatigue*, London, 1908.

THE INFLUENCE OF THE MARGINAL SENSE ORGANS ON METABOLIC ACTIVITY IN CASSIOPEA XAMACHANA BIGELOW

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The results of my earlier studies[1] have shown a marked influence of the marginal sense-organs on the rate of regeneration in *Cassiopea*, when halves of the same specimen are used for comparison. Further experiments on regeneration have confirmed these results, and have shown that when two halves of any medusa disk are subjected to the same operation the amount of regeneration from the two halves is identical in extent within the limits of error of measurement employed

Since the influence of the sense-organs on the rate of regenerations is most marked in the earlier stages of any experiment, several series of disks were (1) separated into halves, and (2) the sense-organs were then removed at different intervals of time after the first operation as shown in the following table:

TABLE 1

Series No.	*First operation Disk cut into 2 half disks*	*Second operation S. O. removed from one-half disk*	*Result*
1	9.45 a.m. July 22	7.00 a.m. July 23	Half with S. O. fastest
2	8.00 a.m. July 24	7.00 p.m. July 24	Half with S. O. fastest
3	7.15 a.m. July 25	8.00 a.m. July 26	Reg. equal
4	8.00 a.m. July 25	4.00 p.m. July 25	Half with S. O. fastest
5	7.00 a.m. July 26	8.00 a.m. July 27	Reg. equal

When the sense-organs were removed from one half disk in less than 24 hours regeneration occurs just as if they had been removed at the time of the first operation. After about 26 hours, however, the removal of the sense-organs from one of the half disks had no apparent influence on the rate of regeneration, which was the same from both halves of any disk.

Influence of marginal sense-organs on loss of weight in starving.—Mayer[5] found that when Cassiopea was starved in sea water from which all food organisms had been removed by careful filtration, the loss of weight could be expressed mathematically by the formula $y = W\ (1-a)^x$, in which W = the original weight, x the number of days of starvation and a a constant, the 'coefficient of negative metabolism.' The value of a in the equation above differs in experiments involving varying conditions as regards light and darkness, presence or absence of regeneration, etc., but in all cases the formula gives a very close approximation to the observed loss of weight.

In my experiments the two halves of a series of disks were compared after they had been subjected to operations which made possible the comparison of the halves of the disks upon one of which the sense-organs remained, while these were removed from the other half disk (active and inactive series); second, the comparison of half disks on one of which the sense-organs remained, while the other, from which all sense-organs were removed, had its muscles activated by a circuit wave of contraction maintained in an endless labyrinth of subumbrella tissue (active and activated series); third, half disks from which all sense-organs were removed, while one of them was activated by a circuit wave of contraction (activated and inactive series).

TABLE 2

	Loss of weight in active and inactive half disks		*Loss of weight in active and activated half disks*		*Loss of weight of activated and inactive half disks*	
Days after operation	*Weight of half with sense-organs*	*Weight of half without S. O.*	*Weight of half with sense-organs*	*Weight of activated half*	*Weight of activated half*	*Weight of half without S. O.*
0	100.00	100.00	100.00	100.00	100.00	100.00
1	75.34	81.81	76.29	79.41	79.47	81.97
2	66.72	71.27	67.18	70.58	70.63	71.84
3	58.54	64.09	59.76	61.99	62.03	63.91
4	55.27	55.41	55.88	57.18	57.21	57.23

In every instance the results follow closely those obtained when the regeneration was used as the standard of comparison. The results of the entire series of experiments are shown in the following tables, in which the original weight of each series of disks is taken as 100 so that the results read as percentages of the original weight.

In the activated and inactive series as well as in the series composed of activated and inactive half disks the visible activity, i.e., muscular contraction was much greater in those members of each series in the tissues of which the circuit wave of contraction was maintained. When compared with half disks under the control of the sense-organs, the rate of pulsation of the activated disks was from 3.5 times as great at the beginning of any experiment to 10 times as great at the end of the first day. During this interval the rate of the half disks with the sense-organs fell to scarcely more than half the original rate while, on the contrary, the rate of the activated half disk always increased.

Simultaneous kineograph records of the pulsations of the two halves of the same disk, one with its sense organs and the other activated by a circuit wave of contraction, were made to measure the amount of muscular work done by each half under the given experimental conditions. In all these determinations it was found that the amplitude of the contraction as recorded on the drum depended upon the character of the operation that had been performed upon the half disk. When the active half remained with its subumbrella muscles undisturbed, the resulting contraction was more extensive than that of the activated mate. When, however, the same operation had been made upon each of the two halves of a disk, from one of which the sense organs were removed, the amplitudes were equal for each half, so that the rate of pulsation is apparently a true measure of the work done.

The comparison of the loss of weight shown by activated and active, as well as by the activated and inactive pairs of half-disks, shows very clearly, just as when regeneration is used as the basis for comparison, that muscular activity is a relatively unimportant factor in determining the metabolic activity of *Cassiopea*.

Influence of marginal sense-organs on total metabolism.—To measure the total metabolism of half disks of *Cassiopea* under the several operative conditions involved in the regeneration and starvation experiments, specimens prepared in the manner previously described for these experiments were placed in closed jars containing known volumes of fresh sea water, and after different intervals of time the amount of CO_2 given off was determined for each specimen. In making these determinations, the records were kept in terms of increased hydrogen ion concentration, the values of which were later determined by adding known volumes of CO_2 to a volume of fresh sea water equal in amount to that contained by the jars used in the experiments with the medusa disks. In many of the experiments the disks were kept in the closed jars until one member of a pair had ceased to pulsate, because of the narcotizing effects of the CO_2, so that it was possible to obtain a

measure of the CO_2 concentration necessary to bring about narcosis. In nearly all instances the activated half disk was the first to succumb, and when it had stopped it did not again pulsate until once more stimulated by an induction shock. The active half disks showed more resistance to CO_2, and when removed from the jars in which they had ceased to pulsate and put into fresh sea water would start pulsating again within one or two minutes, even when they had been inactive for several hours.

When the closed jars, in which the medusae were kept for these experiments, were allowed to remain in the light, the disks would continue to pulsate for several days as the CO_2 would be in part used up by the symbiotic algae which are very abundant in the tissues of *Cassiopea*.

The results of a typical experiment are shown in table 5, in which the half disk with sense-organs is designated "*a*" and the activated half disk "*b*."

TABLE 3

No. of Specimens		Weight in grams	Pulsation Rate 1.45 p.m.	4.40 p.m.	7.30 p.m.	H^+ Concentration
1	a	22.8	44	62	18	7.80
	b	23.0	128	136	120	7.90
2	a	30.0	36	34	22	8.00
	b	30.5	88	86	98	8.00
3	a	39.0	44	36	8	7.90
	b	41.0	130	158	126	7.90
4	a	28.00	56	22	25	7.90
	b	29.00	96	106	Out	8.00
5	a	29.25	84	32	32	7.90
	b	28.75	116	120	132	7.90

The hydrogen ion concentration of the sea water determined at the beginning of the experiment was PH. 8.10 (8×10^{-9}) so that the change brought about in its reaction on account of the activity of the several half disks was from 0.1 to 0.3 of the PH. unit. Using the same volume (1200 cc.) of fresh sea water, it was found that the addition of the 5 cc. of CO_2 would usually bring about a change of 0.2 in the PH. reading, so that each half disk had apparently given off approximately that volume of CO_2, in the 8 hours during which they were in the jars. When the specimens were left for a longer time in the jars the amount of CO_2 given off became proportionately less as time went on as the disks became more thoroughly narcotized, so that when the hydrogen ion concentration of the water had become 7.8 the disks had ceased pulsating. If left for some hours in this water, the nervous system became incapable of transmitting impulses and finally the sense-organs were rendered inactive.

[1] Cary, L. R., these PROCEEDINGS, 1, No. 12; *J. Exper. Zoöl.*, 21, No. 1.
[2] Mayer, A. G., *Publ. Carnegie Inst. Washington*, No. 183.

NEW EVIDENCE IN REGARD TO THE INSTABILITY OF HUMAN TYPES

By Franz Boas

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A number of years ago I carried on, under the auspices of the United States Immigration Commission, an investigation on the physical types of immigrants and of their descendants. One of the results of this inquiry was the establishment of the fact that there is a difference in appearance between the immigrants and their descendants. So far as the bulk of the body is concerned, this information was not new. Analogous phenomena had been observed in 1877 by H. P. Bowditch in Boston, and by Peckham in Milwaukee. It was new, however, that there is also a change in such features as the cephalic index and the width of the face. It was found that on the average the heads of descendants of immigrants of East European types are more elongated, and those of the descendants of South Europeans more rounded, than those of their parents. The data were obtained partly by a generalizing method, partly by a comparison between parents and children.

The results of this inquiry have been attacked by many writers, on the basis that they decline to believe that such changes can occur. I have not found any actual criticism of my method and of the results, except by Corrado Gini, who doubts the inferences drawn in regard to the populations of Italian cities which also show a modification of the cephalic index.

I think the hesitation of many authors to accept the results is due largely to a misinterpretation of their significance. I may be allowed to state concisely here what I think has been proved, and what inferences seem justifiable.

The investigation has a direct bearing upon the question of the classification of human local types, more particularly of European types. Many attempts have been made to give a satisfactory classification of the divergent types that occur in Europe. Pigmentation, stature, form of the head, and form of the face, show material differences in various parts of Europe, notwithstanding the fundamental sameness of the whole race. Authors like Deniker, and many others, have carried out on this basis an elaborate classification of European types in a number of 'races' and 'sub-races.'

In this classification the assumption is made that each race that we find at the present time in its particular environment is an hereditary

type different from the others. In order to express this assumption, I should like to use the term that these races and sub-races represent, 'genetic' types—genetic in the sense that their characteristics are determined by heredity alone. The question, however, has not been answered, whether these types are really genetic types, or whether they are what I might call 'ecotypes,' in so far as their appearance is determined by environmental or ecological conditions. If we include in this term not only environmental conditions in a geographical and social sense, but also conditions that are determined by the organism itself, we might, perhaps, still better call them physiological types, in the same sense in which the biologist speaks of physiological races. My investigation then was directed to the question in how far a certain type of man may be considered a genetic type, in how far a physiological type. If there is any kind of environmental influence, it is obvious that we can never speak of a genetic type *per se*, but that every genetic type appears under certain environmental or physiological conditions, and that in this sense we are always dealing with the physiological form of a certain genetic type. The question, then, that demands an answer, is, in how far genetic types may be influenced by physiological changes.

I believe, that, on the basis of the material that I collected, we must maintain that the same genetic type may occur in various physiologically conditioned forms, and that so far as stature, head-form, and width of face are concerned, the differences between the physiological forms of the same genetic type are of the same order as the differences between the races and sub-races which have been distinguished in Europe. I must add, however, that these remarks do not refer to pigmentation, for, contrary to a widespread belief, we have no proof of environmental influences upon pigmentation. For this reason the classification of European races cannot be considered as proving genetic differentiation.

The whole investigation which I carried on, and certain comparable observations obtained from older literature, do not indicate in any way to what physiological conditions the observed changes may be due. The only physiological causes in regard to which evidence is available relate to the bulk of the body, and to a certain extent to the proportions of the limbs. The size of the body depends upon the conditions under which growth takes place. Growth depends upon nutrition, upon pathological conditions during childhood, and upon many other causes, all of which have an effect upon the bulk of the body of the adult. When these conditions are favorable, the physiological form of a certain genetic type will be large. If there is much retardation during early life, the physiological form of the same genetic type will be small. Retarda-

tion and acceleration of growth may also account for varying proportions of the limbs. On the other hand, we have no information whatever that would allow us to determine the cause of the physiological diminution in the size of the face that has been observed in America, nor for the change in the head-index that occurs among the descendants of immigrants.

Furthermore, there is nothing to indicate that these changes are in any sense genetic changes; that is to say, that they influence the hereditary constitution of the germ. It may very well be that the same people, if carried back to their old environment, would revert to their former physiological types.

In fact, it can be shown that certain features are strictly hereditary, and that, although the physiological form of a genetic type may vary, nevertheless the genetic type as such will exert its influence. Professor von Luschan has repeatedly called attention to this fact as revealed in the modern populations of Asia Minor, where, notwithstanding the mixture which has continued for at least four thousand years, the characteristic Armenian, Northwest European, and Mediterranean types survive in the mixed population. Similar examples may be observed in Italy. I have calculated the variability of the head-form that is found in different parts of Italy, based on the data collected by Ridolfo Livi. The head-form of the North Italians is excessively short. The head-form of the South Italians is decidedly elongated. In between we find intermediate forms. In the Apennines, we have, in addition to the mixture of these two Italian forms, a marked immigration from the Balkan Peninsula, which introduced another short-headed type. As a result of these long-continued mixtures, we observe low degrees of variability in northern and southern Italy, high degrees of variability in the central regions, particularly in the Abruzzi. These indicate permanence of the component types of the mixed population.

During the last few years some new data have been collected that confirm my previous observations. I have pointed out several times that changes of types have been observed in Europe wherever a careful comparison between city population and country population has been made. Generally the changes that occur there have been ascribed to selective influences; but the intensity of selection would have to be so great, that it does not seem plausible that they can be explained by this cause.

In conjunction with Miss Helene M. Boas, I have made a comparison between the head-forms of the city populations of Italy and of the rural population in the areas surrounding the cities, and compared

these data with the information given in the Italian census in regard to the immigration into cities. I found throughout that the variability of head-form in each city is smaller than would be found in a population in which all the constituent genetic types were present without physiological modification. This result has been criticised by Corrado Gini, on the basis that in former times migration was less than what it is now. I grant this point; but nevertheless it is quite obvious, that, although no exact data are available, the mixture of population in a city like Rome or like Florence must be very great, since the political conditions for the conflux of Italians, and even of individuals from outside of Italy, have been favorable for a very long period. If this is true, we should expect a very high degree of variability in Rome, which, however, is not found.

Turning to new data, I wish to mention the observation made by Dr. Hrdlička, who, in a paper read before the Pan-American Scientific Congress, has stated that he found the width of face of Americans of the fourth generation—that is to say, of descendants of Europeans who had no foreign-born ancestor after the fourth generation back—was materially decreased as compared to the width of face found among European types. This conforms strictly with what I found among the descendants of immigrants of all nationalities.

A year ago I had the opportunity to make an anthropometric investigation of a considerable number of natives of Porto Rico. This work was carried on in connection with the Natural History Survey of Porto Rico organized by the New York Academy of Sciences. The population of Porto Rico is derived from three distinct sources—from people belonging to the Mediterranean type of Europe, from West Indian aborigines, and from Negroes. The Mediterranean ancestry of the Porto Ricans leads back to all parts of Spain; but among the more recent immigrants, Catalans, people from the Balear Islands and from the Canary Islands prevail. There are also a fair number of Corsicans. The Spanish immigration has been quite strong even up to the present time. Among the individuals whom I measured, 14% had Spanish-born fathers, some even Spanish-born mothers. From all we know about the history of the people of Porto Rico, we must consider them essentially as descendants of male immigrants who intermarried with native women. It is evident that in early times this must have led to the development of a Mestizo population, in which, however, the amount of Indian blood must have decreased very rapidly owing to the continued influx of Spanish blood, and the elimination from the reproductive series of the male Mestizo element. The Negro population is settled

particularly on the outer coast of the island; while the amount of Negro blood in the interior is apparently not very great, except near the principal routes of travel.

According to European observations, the Spanish ancestors of this population, while living in Spain, are long-headed. The Negro element is of mixed provenience, from many different parts of Africa, but, on the whole, the Negro in Africa is also long-headed. The West Indian element, judging from the few prehistoric crania that have been recovered, represents a very short-headed type. The modern Porto Rican is short-headed to such a degree that even a heavy admixture of Indian blood could not account for the degree of short-headedness. If we apply the results of known instances of intermixture to our particular case, and assume stability of type, we find that, even if the population were one-half Indian and one-half Spanish and Negro, the head-index would be considerably lower than what we actually observe. There is therefore no source that would account for the present head-form as a genetic type; and we are compelled to assume that the form which we observe is due to a physiological modification that has occurred under the new environment. The head-form of those individuals whose fathers were born in Spain is noticeably more elongated than that of the individuals whose parents are both Porto Ricans. The head-index of the Mulatto population is intermediate between the index of the native Porto Ricans and that of those whose one parent is Spanish. The average index of the Porto Rican is 82.5. The average index of the Spaniard in Spain is less than 77. We find, therefore, an increase of five units here, which can in no way be accounted for by genetic considerations.

I may mention in this connection that the average stature of the Porto Ricans is apparently almost the same as that of the Sicilians in New York, and that throughout the period of growth the stature follows about the same curve as that represented by Sicilian children living in America. If anything, the stature is a little lower, and there is no indication of that acceleration of development which is so often claimed to be characteristic of a tropical environment. Undoubtedly poor nutrition, and probably also pathological causes, have a retarding influence here, which might easily be overcome by better hygienic conditions.

It is unfortunate that we have no accurate statistics of Porto Rican immigration and emigration, which would enable us to state with much greater definiteness what genetic type should be expected here. There is a popular belief in Porto Rico that in certain parts of the island, in

the so-called 'Indiera,' Indian types have persisted to a greater extent than elsewhere. I have not been able to find any definite indication of a difference in type; but I have measured only a few individuals from these districts. The material that I have been able to study comes from all parts of the island, but principally from the western-central part. The phenomena here described occur with equal intensity in all parts of the island.

The question of the degree of instability of human types seems to my mind an exceedingly important one for a clear understanding of the problems of physical anthropology. It would be particularly desirable to study the problem among immigrants living in different rural communities of the United States, and it would be even more desirable to have information in regard to the types that develop among the East Europeans and South Europeans who return to Europe and settle in their old geographical environment.

A REVISION OF THE ATOMIC WEIGHT OF TIN

By Gregory Paul Baxter and Howard Warner Starkweather

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A recent investigation upon the atomic weight of tin by Briscoe,[1] in which stannic chloride was compared with pure silver, yielded a very concordant series of results, with an average value 118.698 (Cl = 35.457). This value has been adopted by the International Committee on Atomic Weights in preference to that found by Bongartz and Classen,[2] 119.0, which has been in general use for some time. Since the electrodeposition of cadmium and zinc in a weighed mercury cathode has been found to be a process capable of great accuracy,[3] and since tin amalgam promised to be unusually well adapted for quantitative handling, this electrolytic method has been applied to the analysis of stannic chloride also.

In brief the method of operation was as follows: Pure tin was converted to tetrachloride by treatment with pure chlorine, and the tetrachloride was purified by fractional distillation. After weighed portions of the chloride had been dissolved in dilute hydrochloric acid, the metal was deposited electrolytically in a mercury cathode contained in a weighed glass cell similar to that previously described.[4]

Before converting the tin to tetrachloride it was freed as far as possible from acid-forming elements by twice transporting the metal electrolytically through an acid solution of stannous chloride, the anode

being an ingot of tin. This served at the same time to eliminate a large part of the copper and lead which were the chief metallic impurities. The product was fused in hydrogen on an alundum boat.

In preparing the tetrachloride special pains was taken to avoid exposing the substance to moisture. The ingots of metal were placed in a glass tube connected at one end with an apparatus for producing dry chlorine and at the other with a glass bulb containing a small quantity of tin. After the tube and bulb had been exhausted dry, chlorine was admitted and passed over the metal until it had been almost completely converted to chloride. The product was allowed to stand in contact with tin for some time to remove excess of chlorine and reduce lead tetrachloride, and was then fractionally distilled in vacuum at room temperature by condensing the product in bulbs cooled either with alcohol and solid carbon dioxide or with liquid air. First the most volatile fraction was removed with liquid air and rejected. The remainder was four times distilled, with rejection of a residue of about 15 grams in each case. Then two more fractions were eliminated with liquid air. The remainder was finally collected for analysis in a series of small bulbs. In the course of the distillation each fraction was sealed off from the rest of the apparatus as soon as collected. Two series of preparations were made, the procedure differing only slightly in the two cases.

The whole apparatus was constructed of glass, with sealed joints, the only glass stopcock was located between the tube containing the tin and the chlorine generator, and was lubricated with syrupy phosphoric acid. In place of stopcocks special glass valves devised by Briscoe were employed, in which a fused seal was broken when it was desired to open the valve.

The experimental procedure of an analysis was as follows: The glass bulb containing the stannic chloride was weighed in air, and again under water to find its displacement. Next the bulb was broken under about 75 cc. of 0.03 normal hydrochloric acid. The solution was then filtered into the weighed cell containing about 200 grams of mercury, and the glass was thoroughly washed with hydrochloric acid of the above concentration. The filter with the glass was burned in a weighed platinum crucible. Blank tests showed no appreciable quantity of glass to be dissolved in the process. The more concentrated washings were added to the main solution in the cell, the more dilute were evaporated with nitric and sulphuric acids in quartz. After nearly all the tin had been electrolytically deposited from the main solution, the residual electrolyte was concentrated, together with the evaporated wash-

ings, and the whole further electrolyzed. At the completion of the electrolysis the electrolyte was displaced successively with water and pure alcohol, both of which had been freshly saturated with hydrogen. To dry the amalgam the cell was placed in a small desiccator and the pressure reduced as far as possible. The electrolyte was evaporated, at first in a quartz dish, later in a weighed platinum crucible, and the residue heated gently. The weight of tin, computed upon the assumption that the residue was stannic sulphate, was added to that found in the amalgam. Although complete precipitation of the tin was never secured, the residue usually did not exceed 2 mg. in weight, and was never as much as 4 mg. Repetition of the electrolysis, after dissolving the residue in dilute sulphuric acid and returning it to the cell, sometimes caused a slight decrease in the weight of tin recovered. In all cases constant weight within 0.1 mg. was finally secured.

As a check upon the accuracy of this method of determining tin, in two experiments a weighed ingot of pure tin was electrolytically transported from the anode to the mercury cathode of the weighed cell. In one case a gain of 0.01 mg., in the other a loss of 0.06 mg. was observed. Such small changes are less than the probable error of weighing the cells.

The experimental data and the results calculated from them (assuming the atomic weight of chlorine to be 35.457) are shown in the following table. The weights of the bulbs containing the stannic chloride, of the glass and of the tin dissolved in the mercury are all corrected to vacuum.

Series	*Fraction*	*Grams of* $SnCl_4$	*Grams of* Sn	*Ratio* Sn:Cl_4	*Atomic weight of* Sn
I	5	11.64269	5.30498	0.837050	118.717
I	7	13.5943	6.1935	0.83687	118.691
I	8	10.0897	4.5971	0.83696	118.705
I	9	11.4319	5.2080	0.83677	118.678
I	11	12.2869	5.5983	0.83699	118.709
I	12	12.20889	5.56286	0.837020	118.713
I	13	10.7469	4.8965	0.83695	118.703
I	14	11.54233	5.25846	0.836818	118.684
			Average.....	0.836929	118.700
II	2	15.65437	7.13198	0.836852	118.689
II	3	16.23310	7.39664	0.837059	118.718
II	4	17.29151	7.87850	0.836980	118.707
II	7	15.04889	6.85695	0.837037	118.715
II	8	18.36074	8.36507	0.836869	118.692
II	9	21.58929	9.83676	0.836991	118.709
II	10	17.22210	7.84718	0.837040	118.716
II	11	15.70516	7.15589	0.837018	118.713
			Average.....	0.836981	118.707
				0.836955	118.703

The results for the atomic weight of tin are arranged in the order in which the fractions were distilled, not in the order of analysis. Since only slight irregular variations are apparent, there can be little question that the different portions were identical in composition.

The final average, Sn = 118.703 (Cl = 35.457) is in very close agreement with the one obtained by Briscoe by comparison with silver, 118.698. The percent of tin in stannic chloride found by us is 45.562, while Briscoe found 54.439% of chlorine. The sum is 100.001%, a highly satisfactory and convincing outcome.

It is a great pleasure to express our gratitude to the Carnegie Institution of Washington and to the Elizabeth Thompson Science Fund for very generous assistance in providing indispensable apparatus.

[1] *Trans. Chem. Soc.*, **107**, 63 (1915).

[2] *Ber. D. Chem. Ges.*, **21**, 2900 (1888).

[3] Baxter and Hartmann, *J. Amer. Chem. Soc.*, **37**, 113 (1915). Baxter and Grose, *Ibid.*, **38**, 857, 868 (1916).

[4] Baxter and Hartmann, these PROCEEDINGS, **1**, 26 (1915).

FURTHER STUDIES OF NERVE CONDUCTION IN CASSIOPEA

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Studies of recent years have shown the importance of hydrogen ion concentration in determining the rate of nerve conduction in *Cassiopea*. Ordinary distilled water often remains acid even though air freed from CO_2 by means of soda-lime has been bubbled through it for 72 hours. Accordingly, Prof. George A. Hulett kindly arranged to have 144 litres of distilled water prepared in accordance with his well known method (Ueber die Reinigung des Wassers durch Distillation, *Zs. phys. Chem.*, **21**, 287, 1896) in his laboratory at Princeton University. This water was sealed in 144 pyrex glass flasks and thus transported to Tortugas. The hydrogen ion concentration of each flask was tested separately, the range being 0.8 to 1.0 $\times$ 10^{-6}, and the average being 0.9 $\times$ 10^{-6}, or 6.04 PH.

Fifty litres of this water were placed in a green glass carboy which had previously held Merck's distilled water; and air freed from CO_2 was bubbled through it at an active rate for 78 hours, after which the water in the carboy had a PH. of 8.0 which it maintained for eight days while 139 experiments were made with it. The alkalinity then declined to 7.5 PH. while 26 other experiments were made; the average for the

series being 7.93 PH. or 1.17×10^{-8} hydrogen ion concentration. Its alkalinity was probably due to soda derived from the glass carboy, the balance being maintained by a tendency of the water itself to become acid through leakage of CO_2 from the air. Professor J. F. McClendon found the PH. of the Tortugas sea water to range from 8.1 to 8.22, and dilution with alkaline distilled water of 7.93 PH. maintained the normal hydrogen ion concentration of the sea water even when diluted with its own volume of such distilled water.

TABLE I

ILLUSTRATED BY FIGURE 1

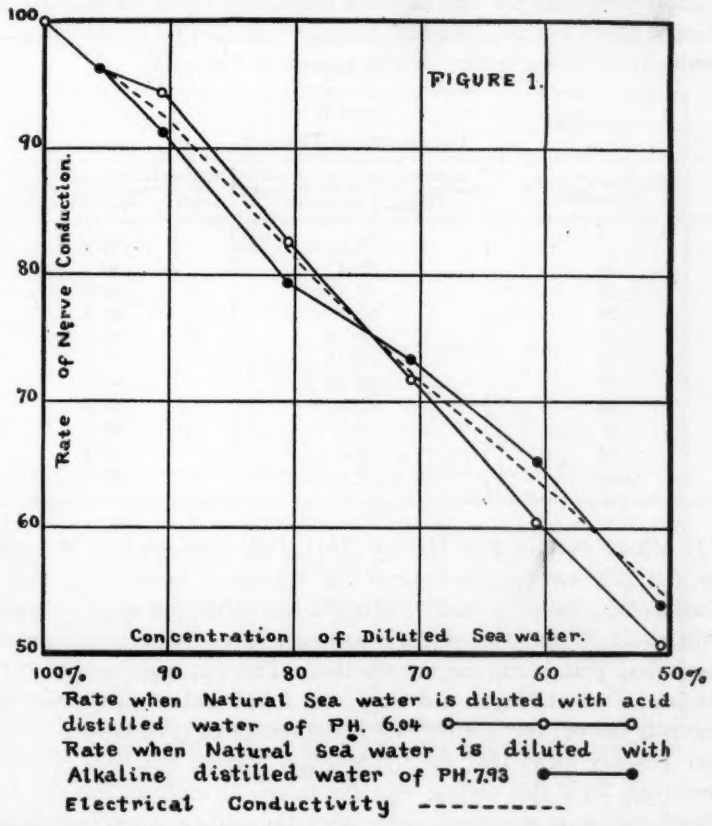
COMPOSITION OF THE SOLUTION	I RATE OF NERVE CONDUCTION WHEN THE SEA WATER IS DILUTED WITH ACID DISTILLED WATER OF 6.04 PH. AT 30°C.	PROBABLE ERROR	II RATE OF NERVE CONDUCTION WHEN THE SEA WATER IS DILUTED WITH ALKALINE DISTILLED WATER OF 7.93 PH AT 30°C.	PROBABLE ERROR	III RELATIVE ELECTRICAL CONDUCTIVITY OF TORTUGAS SEA WATER DILUTED WITH DISTILLED WATER OF 7.8 PH. AT 30°C.
Natural sea water of 8.1 to 8.22 PH.	100.00		100.00		100.00
95 cc. sea water + 5 cc. distilled water	96.43	0.74	96.23	0.67	
90 cc. sea water + 10 cc. distilled water	94.38	0.88	91.44	1.00	92.16
80 cc. sea water + 20 cc. distilled water	82.68	1.16	79.51	1.18	81.38
70 cc. sea water + 30 cc. distilled water	71.95	1.13	73.91	1.16	71.53
60 cc. sea water + 40 cc. distilled water	60.41	0.59	65.72	1.01	64.26
50 cc. sea water + 50 cc. distilled water	50.83	0.90	54.16	0.79	54.08

It will be seen by comparing columns II and III of the above table that when alkaline distilled water of about 8 PH. is used to dilute the sea water the decline in rate of nerve conduction is practically the same as the decline in the electrical conductivity of the sea water when similarly diluted. We should remember, however, that the concentration of the sodium, calcium and potassium cations declines in practically the same ratio and thus the rate of nerve conduction may be proportional to the concentration of these cations rather than to the electrical conductivity of the diluted sea water as a whole.

However, Prof. Ralph S. Lillie is right in a recent paper (*Amer. J. Physiol.*, **41**, 123) wherein he states his belief that the rate of nerve conduction in *Cassiopea* in diluted sea water does not decline in accord

with Freundlich's law of adsorption as I had erroneously supposed (these PROCEEDINGS, **1**, p. 270; **2**, p. 37).

The distilled water used in my previous experiments was slightly acid and thus stimulating in slight dilution and depressant in stronger dilutions (see Table I, column 1) thus giving the semblance of an adsorption curve. Nor did I realize the effects of slight changes in tem-



perature in the various solutions, for the effect of 0.1°C. is readily appreciable.

Lillie advocates an extension of the theory of Faraday and de la Rive, that the transmission of the excitation state from the immediate site of activity to the adjoining resting area is dependent on an electrical local action of the same essential nature as that which is responsible for the etching or corrosion of non-homogeneous metallic surfaces, such as iron

in contact with an electrolyte solution; and if this be true, the rate of nerve conduction should be a function of the electrical conductivity of the medium, and also of the surrounding fluid.

To further test this hypothesis, I determined the electrical conductivity of Tortugas sea water when heated or cooled, and compared it with the rate of nerve conduction at corresponding temperatures.

This shows that the rate of nerve conduction has a temperature coefficient about two and one-half times as high as that of the electrical conductivity of sea water; as will appear in Table II.

TABLE II

ILLUSTRATED BY FIGURE 2

TEMPERATURE C° OF THE SEA WATER	RATE OF NERVE CONDUCTION IN CASSIOPEA. MEAN OF HARVEY, MAYER, AND CARY'S OBSERVATIONS	RELATIVE ELECTRICAL CONDUCTIVITY OF TORTUGAS SEA WATER DETERMINED BY KOHLRAUSCH'S METHOD
23	71.3	88.9
24	76.3	90.7
25	81.17	92.6
26	85.8	94.4
27	90.74	96.2
28	95.47	98.0
29	100.00	100.0
30	104.47	101.6
31	109.2	103.5
32	113.4	105.3
33	117.8	107.1

It will be recalled that Harvey (1911, *Publ. Carnegie Inst. Washington*, No. 132) was the first to show that the rate of nerve conduction in *Cassiopea* augments in nearly a straight line as the sea water is heated until about 36° to 38°C. where it suddenly falls off, thus giving a curve resembling that of an enzyme reaction. This has been confirmed by the later work of Mayer, and Cary; and I find that when the rate has begun to fall off, the original rate is not recovered upon cooling. This may possibly mean that the hypothetical enzyme has been partially destroyed; for if this decline were due merely to asphyxiation the rate should recover to a greater degree when the medusa is replaced in sea water of normal temperature. Moreover, the high temperature coefficient of the rate of nerve conduction suggests that we may be dealing with a chemical reaction in which a compound is formed composed of sodium, calcium, and some proteid element; the degree of ionization of which is considerably affected by temperature in the manner suggested by Hardy, 1900; Quincke, 1902; and Bayliss, 1915, *Principles of General Physiology*, p. 77.

Possibly, also, the negative electrical potential associated with the wave of nerve conduction may increase the surface tension of the alkaline colloidal particles thus reducing their size, rendering them more soluble and thereby increasing the concentration of the reacting ions.

Contrary to Lillie's hypothesis, we have direct evidence that the rate of nerve conduction may be independent of the electrical conductivity of the solution surrounding the nerve, for I have found (these PROCEEDINGS, 1, p. 270) that if sea water be diluted with 0.415 molec-

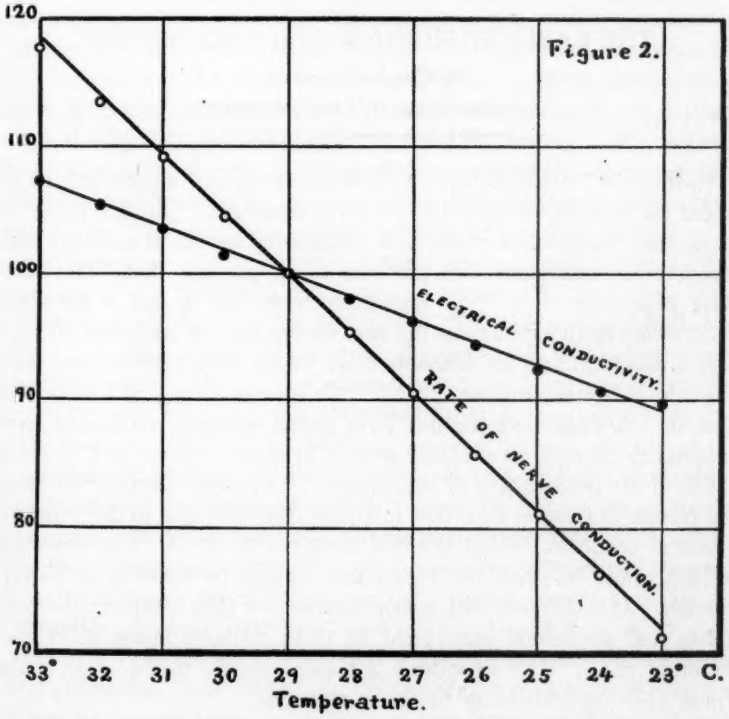


Figure 2.

ular $MgCl_2$ the rate of nerve conduction is only slightly more depressed than if the sea water is diluted with distilled water, or with dextrose; yet the $MgCl_2$ maintains a nearly normal electrical conductivity, while with distilled water or dextrose it declines in nearly the same ratio as the dilution. Nor do the experiments I have made with solutions containing some but not all the cations of sea water support Lillie's view. Thus if the rate of nerve conduction in 0.647 molecular NaCl is 55, it becomes 100 in 85.3 cc. of 0.6 molecular NaCl + 14.69 cc. of 0.39 molec-

ular $MgCl_2$. Here the electrical conductivity of the solution is somewhat reduced while the rate of nerve conduction is much augmented. This is, of course, a striking instance of Loeb's law of the antagonism between a univalent and a bivalent cation; even though the bivalent cation in this case is magnesium, well known to be a depressant especially for muscular activity in *Cassiopea*.

A full report will appear in papers from the Department of Marine Biology to be published by the Carnegie Institution of Washington.

THE EARLIEST FRESH-WATER ARTHROPODS

By Charles Schuchert

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In the year 1900 Prof. T. C. Chamberlin published a prophetic paper under the title *On the habitat of the early vertebrates*.[1] In this paper he holds that the problem which he is considering admits of no other than hypothetical treatment, and proposes the hypothesis that "The chordate phylum is . . . . essentially from first to last a terrestrial race, whose main habitat was the land waters and the land itself, though still a race that sent its offshoots down to sea from time to time from the mid-Paleozoic onwards " (412). He arrives mainly at this conclusion on the basis that the fish form among animals could only have originated through mechanical genesis in swift streams and under a mode of life independent of the bottom. The most essential mechanical feature of rivers is their flow in a fixed direction, and to this insistent physical condition animal life had to adapt itself, for "otherwise the animal would be swept out to sea and its race be ended as a stream-dweller. It is different with ocean currents, for they return upon themselves and an animal may yield to them without losing its marine habitat" (406). This hypothesis is applied mainly to the origin of the fishes and less insistently to the eurypterids.

In regard to the origin of the eurypterids Chamberlin states: "From the occurrence of eurypterids first in marine beds apparently and later in fresh-water deposits it has been inferred that they were originally sea-dwellers and later became adapted to land waters, but the meagerness of their marine record on the one hand, and their abundance and fine preservation in the fresh-water deposits on the other, give point to the question whether their early marine record is anything more than the chance deposit of river forms borne out to sea" (403). But "it may be equally true that the fish and the eurypterids descended

from the rivers to the sea in the mid-Paleozoic, though their remote ancestors may have ascended from it" (404). Finally, that the lands were clothed with vegetation even in Proterozoic time and that therefore there was an abundance of food in the rivers is attested in part by the great quantity of carbonaceous material in the strata of this time, plus the complete disintegration of the crystalline rocks as is shown by the fine aluminous muds, the clean sandstones, the great amounts of limestone, and the localized beds of iron-ore. Although arkoses are present (broken up and undecomposed granites), their mass is insignificant in comparison with the quantity of the other sedimentaries.

Early in 1916 Prof. Joseph Barrell also considered the probable first habitat of fishes, but from the standpoint of their occurrence as fossils in the rocks, in papers entitled *Dominantly fluviatile origin under seasonal rainfall of the Old Red Sandstone* and *Influence of Silurian-Devonian climates on the rise of air-breathing vertebrates.*[2] He agrees with Chamberlin and concludes that it is probable "that fishes arose in land waters" and "as such they constituted primarily a river fauna." The lung-fishes arose under semiarid climates and seasonal waters. "The exposure of the tidal zone alternately to water and to air had, then, nothing to do with the origin of lungs." "The evidence is regarded as strong that the air-bladder was originally developed as a supplemental breathing organ, although in modern fishes it has been mostly diverted to other uses. Among certain Devonian fishes, living under more and more strenuous climatic conditions of seasonal dryness, the use of the air-bladder for respiration became essential, and with the diminishing availability of the waters of certain regions the gills in those species which survived this crisis in evolution became correspondingly atrophied. The amphibians thus arose under the compulsion of seasonal dryness." Finally, "Climatic oscillation is a major ulterior factor in evolution" (388–391).

Later in 1916 appeared a suggestive memoir entitled *The habitat of the Eurypterida*, by Dr. Marjorie O'Connell,[3] which has stimulated the writing of these remarks. This work seeks to point out the actual habitat of eurypterids during the Paleozoic, as derived from a study of their entombment in all places Paleozoic, but chiefly in America and Europe. It is replete with paleontologic information, and all of the more important occurrences of these animals and their immediate associates are described in as much detail as the author was able to glean, in the main from the widely scattered literature, though she is also familiar with the actual field relations in the state of New York. There are in all the world about 130 described species in 14 genera, with the acme of

development in the upper part of the Silurian where about 40 forms are known. There is very little evolution after Devonian time. On page 9 of Doctor O'Connell's memoir is given in italics the summation of her studies: *The eurypterids throughout their entire phylogenetic history lived in the rivers.* Lest the reader forget this thesis it is repeated on many subsequent pages. Though the book is argumentative and positive, and at times pleading in behalf of its theme, prying into and tearing apart all things eurypterid, the style remains essentially scientific.

It has been and still is widely held that eurypterids were marine animals previous to the Devonian, that toward the close of the Silurian they became euryhaline or able to live in both salt and brackish water, and that after Silurian time they probably became wholly restricted to the fresh water of the lands. Even though they were regarded as marine animals, living associated with trilobites and brachiopods, it has long been a difficult problem to explain why their remains are so very rare in the normal marine faunas of the Upper Cambrian, Ordovician, and Silurian, and even when they do occur are almost always in fragments. Although Doctor O'Connell points out this condition of occurrence, she does not make of it the strongest point in her argument, and yet it is the one fact that seemingly cannot be explained away. On the other hand, the entire specimen of *Strabops thacheri* at Yale has attached to the slab two specimens of the brachiopod *Obolus lamborni* and a poor head of a trilobite; the entombment is in a magnesian limestone, and because of the marine fauna of the same formation as listed by Beecher, one still retains the impression that this oldest and most generalized eurypterid may at least have been an inhabitant of the sea if not born of it. In the earliest deposits of the Silurian (Shawangunk) the eurypterids usually occur in fragments and in young specimens in thin and localized black shales, interbedded in a very thick and regularly bedded sandstone series, the material of a delta spreading into the epeiric sea. These beds are otherwise almost unfossiliferous, having associated with the eurypterids only Arthrophycus, a lobworm-like burrow that in the Medina formation is associated with marine fossils, showing that the Shawangunk is not a river flood-plain deposit but the sands of a delta under the influence of marine waters. On the other hand, in the latest Silurian formations of America, which are clearly not of normal marine waters, the eurypterids either have no marine associates or there are scattering specimens of cephalopods (Orthoceras, Trochoceras), bivalve crustaceans (Leperditia), and brachiopods (Lingula); in Bohemia, the middle Silurian, with a normal marine fauna of many species, has seven to eight forms of Pterygotus and one of Slimonia,

but all of the specimens are in fragments. On Oesel, however, the three or four species of eurypterids are beautifully preserved in fine-grained gray dolomites or dolomitic limestones, with the original chitin of the animals still preserved; in fact, they have been etched out of the matrix and the tests mounted in Canada balsam. Here (at Attel) they are associated with an abundance of *Leperditia angelina*, *Orthoceras tenue*, and great stony heads, 2 feet across, of the hydroid Clathrodictyon (Twenhofel 1916). Surely these deposits are more of marine origin than of fresh waters.

All of this evidence, however, the present writer grants, does not necessarily prove that the eurypterids were born of the sea and continued to live in this environment until the close of the Silurian, for if this was their habitat, surely we should get good remains of them oftener than we do in the many normal marine formations of the Paleozoic, and especially in the Trenton and Niagaran formations. It would seem that, if the eurypterids were wholly marine animals in the Ordovician and especially in the Silurian, they should be as common and as well preserved as, for instance, the marine trilobites. This is not at all the situation. On the other hand, the evidence does not exclude the eurypterids from having also lived, at least at times, in brackish waters, the embayments of deltas, a conclusion that Doctor O'Connell will have none of. Nevertheless, we shall have to accept her main conclusion that the eurypterids were fresh-water animals, but with this modification, that they also appear to have lived at times in the brackish waters of more or less large bays and possibly in limited numbers even in the seas, just as some of the fresh-water fishes of the past have gone to sea. Whether they were born of the fresh waters or the ocean still remains a debatable question, though the evidence appears to favor strongly the former habitat.

It is a well-known fact that ceratiocarids and especially ancestral limulids and apodids are either very rare or occur locally in much restricted beds, and these, like the eurypterids, appear to have been entombed in the marine deposits near shore. The sporadic occurrences of the eurypterids and their great rarity as good specimens in normal marine deposits lead Doctor O'Connell to believe that these animals are of fresh-water habitats and that they were floated by the Paleozoic rivers into the open sea-ways just as we see the land animals and plants of today transported into the seas and oceans. On the other hand, her idea of delta deposits appears to be that they are either wholly of the land or of the sea, and her sketch of the late Silurian Bertie and Herkimer deltas (page 116) bears out this too rigid conclusion. More-

over, the illustrations of the Mississippi delta in our text-books are on so small a scale as to give an erroneous conclusion as to the amount of the river flood-plain area. A large-scale map shows that 50% or even more of this delta is under the waters of the Gulf of Mexico, and in areas like Lakes Ponchartrain and Borgne the largest of eurypterids may have dwelt. Hence it will always be very difficult to separate the deposits of such areas, with their mixed biotas, from those of the river flood-plains on the one side, and on the other from those of the more or less marine bays.

In this connection we should not neglect to state that nearly all the eurypterids occur in strata of times when the continents were largest and when the rivers were rejuvenated to stronger and longer flow into the interior seas. Also that Eurypterus lived from Ordovician into Permian time, and Pterygotus was distributed almost throughout the world during the Siluro-Devonian. However, at the times when the seas have their greatest spread we find almost no merostomes of any kind. These are significant facts bearing upon the probable habitats of the Paleozoic merostomes.

Since it appears that the eurypterids are probably in the main fresh-water animals, and as some of them take on the form of scorpions and may therefore be spoken of as river scorpions, this admission opens a most wonderful vista into the probable life of the land during early Paleozoic times, as far back at least as the Upper Cambrian. There are other merostomes of the order Limulava (two genera) in the Middle and Lower Cambrian, and these antennate forms are more directly related to the eurypterids. Of the Synxiphosura, forms ancestral to the Xiphosura, the Middle and Upper Cambrian have at least four genera. As all of these animals also appear to have lived in fresh water, it would seem that the rivers of Cambrian time were peopled by merostomes none of which exceeded 6 inches in length, but in late Silurian time they were not only far more varied in form, but Pterygotus at least, the most active and predaceous of them all, attained a length estimated at from 6 to 9 feet. Stylonurus, the great spider-like eurypterid of the Devonian rivers, may have attained a similar length. In regard to these and other eurypterids, Clarke and Ruedemann,[4] the authorities on these arthropods, think that most of our museum specimens are immature individuals, and often the casts of tests, conditions further favoring the theory that they are fresh-water animals whose thin, light tests were easily drifted by the rivers into the estuaries and the seas. The considerable variety and the great size attained by some forms means that there was an abundance of food, both animals and

plants, in the rivers of Paleozoic time. Some of the animals were the smaller eurypterids, in the Ordovician and Silurian the armored fishes (the ostracoderms), in the Silurian the additional lung-fishes and diminutive primitive sharks (the acanthodians), and in the Devonian, large bivalve molluscs. However, there appear to have been still other arthropods present, such as the ceratiocarids, a stock of phyllocarids, the ancestral limulids (Synxiphosura), and probably also the branchiopod apodids. In the final analysis we must further admit that all were more or less directly dependent upon an abundance of plant food. and therefore that the early Paleozoic lands were clothed with vegetation, none of which is as yet certainly known to paleontologists. In this connection, however, we must also point out that today in no fresh waters are invertebrates of any kind known attaining to 9 feet long or even half that length, although it takes living Limulus eight years to attain to sexual maturity. This implies that at least some of the Silurian and Devonian eurypterids reached a great age, and, living in the rivers, and maybe at times in the brackish-water bays, were the monarchs of their environment. On the other hand, if the smaller merostomes had lived in the sea, as did the trilobites, they would also have been fed upon by the armored cephalopods, the nautilids, and we may have evidence of this preying in the common association of Orthoceras with the eurypterids. Having no enemies to feed upon them in the rivers, and none that were more agile, more powerful, or more cunning until Devonian time, the eurypterids of the Silurian and early Devonian continued to live on for a long time and so attained to a far greater size than any of the fresh-water invertebrates of today, which are dominated by the more active and intelligent river fishes. With the ascendancy of the fishes beginning in the Devonian, we see a diminution and lack of structural change in the eurypterids and the trilobites, and the vanishing of both stocks in the Permian.

The argument of faunal associates and their entombment, plus the nature of the deposits, used by Doctor O'Connell to prove that the eurypterids are fresh-water animals, will also apply to the ceratiocarids, the limulids, and the apodids. In the Middle Cambrian, Walcott has collected three genera of ancestral limulids and other merostomes (Limulava) in blue to black muds rich in kaolin, which were "probably laid down in a small bay or lagoon in close connection with the shallow Middle Cambrian sea." In these deposits trilobites of the genera Agnostus, Microdiscus, Neolenus, Ptychoparia, etc., are fairly common.

It may therefore be that the Limulava, the ancestral limulids, and the apodids were in this case also fresh-water animals drifted by the

streams into the lagoon. If so, this is evidence of fresh-water life as far back as the Lower Cambrian. Further, in the uppermost Cambrian at one locality in Wisconsin the present writer has collected a few fragments and one entire specimen of the ancient limulid Aglaspis in association with an abundance of trilobites, fragmented and entire, and many well preserved brachiopods (*Westonia stoneana*). In all later Paleozoic deposits the limulids are almost always associated with eurypterids, and practically never in normal marine faunas. They are always very rare fossils until early Pennsylvanian time, when *Euproops danæ* is common in that most interesting mixed estuarine and terrestrial fauna preserved in the nodules of Mazon Creek, Illinois. The Triassic limulids also appear to have lived in fresh water, but since late Jurassic time Limulus has been in the sea, and is represented today by several living species in the Atlantic and Pacific oceans. Can, therefore, the annual return of these animals to the land to lay their eggs be a surviving instinct and an ontogenetic expression of their earlier, fresh-water habitat?

In this connection should be recorded a most interesting observation of Limulus made by Professor Barrell many years ago. He took home in the spring of the year a large female Limulus that he had picked up on the shore of New Haven Bay, and kept it in the grassed yard back of the house in which he lived. Under these conditions the animal was still able to crawl about feebly for two days and gave evidence of life for about a week, during all of which time it was out of the water. This clearly indicates that the gill-books are fairly easily adaptable to air-breathing, and the question may be asked whether in the Paleozoic during times of drought ancestral limulids of the fresh waters may not have lived over the dry season, as do the lung fishes of today, buried in the mud and breathing the air.

Attention should also be directed to the occurrences of the Apus-like branchiopods, the oldest of which is Protocaris of the Lower Cambrian, known in a single specimen associated with trilobites in a marine deposit laid down, however, near the shore. In the Middle Cambrian black shale lagoon deposits (Burgess), there are three genera, and then we have no record of them again until Apus is met with in the Triassic—henceforth a persistent synthetic genus, which in the living world is restricted to the more or less evanescent land waters of many continents.

If the eurypterids and limulids arose in the fresh waters, as appears probable, we can then the more readily explain why they and the terrestrial scorpions do not pass through a crustacean nauplius stage, for they had to adapt themselves to the lands or to waters limited in dis-

tribution and more or less evanescent in continuity, to the streams that flowed constantly in one direction and eventually into an unnatural habitat, the sea. They needed a more direct and quicker embryonic development, requiring, it would seem, the abandonment of the nauplius stage. It may therefore well be that the trilobites retaining the nauplius stage did not give rise to these stocks, as is sometimes assumed. We may have to look for this ancestral stock in one still more primitive, and the Protocaris-Apus line of branchiopods suggests itself, but whatever the stock, it would seem to have permanently invaded the rivers of the land either in Proterozoic time, or that postulated intermediate stage in the earth's history previous to the Cambrian but of which we have not a trace of direct evidence, the Lipalian time of Walcott.

[1] T. C. Chamberlin, *Jour. Geology*, 8, 400-412 (1900).
[2] J. Barrell, *Bull. Geol. Soc. America*, 27, 345-436 (1916).
[3] M. O'Connell, *Bull. Buffalo Soc. Nat. Hist.*, 11, No. 3, 1-277 (1916).
[4] J. M. Clarke and R. Ruedemann, The Eurypterida of New York, *Mem. New York State Mus.*, No. 14 (1912).

OBSERVATIONS UPON TROPICAL FISHES AND INFERENCES FROM THEIR ADAPTIVE COLORATION

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The conception that species have been multiplied by divergent evolution of related strains is based upon a great body of verifiable observations. Sound judgment has not been exercised consistently, however, in the attempt to establish the fact that their development has been directed throughout by natural selection.

If the Darwinian hypothesis is true, the characters of organisms should be largely of an adaptive sort, but its adherents have failed, upon the whole, to distinguish between shadow and substance, and have been content to support their position by *imputing* utility to structures and habits, when nothing less than rigorous proof of the fact will suffice. It is not demonstrated, for example, that any class of markings serves for purposes of recognition, or for signalling between individuals of one species. Neither is it proved that some color combinations warn off possible enemies, nor, indeed, that any type of pigmentation is functionally conspicuous. The last assumption, nevertheless, underlies a series of suggestions whose apparent conformity with its terms is held to support the hypothesis of natural selection.

Under the auspices of the Tortugas Marine Laboratory of the Carnegie Institution of Washington, I have been enabled to study many of the bright-hued fishes of the West Indian region, and have attempted to place the facts regarding their coloration upon an objective basis.

Countershading appears almost universally upon these animals. That is to say, their pigments which are externally visible are definitely graded from darkest on the mid-dorsal, or upper, to lightest on the mid-ventral, or lower line. Exceptions to the rule occur only among species of unusual habit or peculiar form.

The systems of pigmentation indicated is no immediate effect of exposure to light, although it involves the production of dark shades in any region in direct proportion to the average intensity of the illumination of that part. This is shown clearly by certain cases of sexual dimorphism in the color of crabs. In the *Portunidae* the abdomen of the ovigerous female is exposed in dorsal view. In correlation with this family character the sexes differ in the coloration of that organ long before sexual maturity, when for the first time its position differs in the two. The abdomen of the male agrees in color with its sternum, but when that of the female is elevated it extends the pigment of the carapace posteriorly over the egg-mass and down into the shadow with delicate countershading. To explain this fact one may apparently appeal only to natural selection, or to sex-limited inheritance of the effect of exposure through many generations.

Other points of interest concern the striking color changes of thirteen species of fishes, which depend upon the color of the objects surrounding the animals. Further study will extend the list. These statements are based upon many records, of which a few were made under laboratory conditions. The greater number by far refer, however, to unconfined specimens studied from a boat, or from the bottom with diving equipment. The various phases of those which will gather about food provided for them may be induced at will by leading the creatures from place to place whose dominant colors differ. Changes of others obey the same laws and may be forecast with precision though they are not demonstrable with the same ease in uncontrolled individuals. Pictures taken with a submarine camera in ten feet of water record some of the observed changes, and show that their general effect is to reduce the conspicuousness of the animals that display them.

The distribution of colors among the various species examined has been investigated in a third phase of the research, and evident correlation of color with habit has been demonstrated. The results obtained may be expressed in brief as follows:

Those fishes in whose coloration red normally predominates are nocturnal, and, in proportion to their numbers, are rarely seen by day.

Gray appears with very different frequency among reef-ranging species and those whose diurnal activity is centered among the coral heads. The ratio of its occurrence in the two cases is roughly commensurate with that of its appearance in the environment of the contrasted groups. Among thirty-one reef-rangers, to state the facts in detail, fourteen show evident adaptive gray color phases or have permanent gray markings, while the same is true of only three of twenty-one species which remain near the coral heads by day. This is equivalent to saying that according to present information gray markings or color phases are about three times as common among reef-ranging fishes as they are among those which live close to the coral heads.

Brown appears so frequently in combination with gray in the patterns of fishes which adapt their coloration almost instantaneously to gray or brown bottoms, whenever the character of their surroundings changes, that in default of special evidence to that effect no functional conspicuousness may be imputed to either color alone, or in combination with the other. The same seems to be true of yellow.

Among the Tortugas fishes the lighter blues at least are correlated with the habit of swimming habitually well above the bottom in water of moderate depth. These tints are peculiarly inconspicuous in the eyes of an observer at a lower level, and photographs of fishes banded with other colors show that the effect of the blue is to blot out its possessor's contour under that condition, since at a distance of a few feet the blue-gray elements in patterns are indistinguishable from the color of the watery background.

Finally, two-thirds of the species seined upon the green grass-flats along shore, and with any show of reason considered typical members of the bionomic association inhabiting such places, are wholly or largely of a green color, or regularly show a green color phase amid green surroundings. If the forms that swim at a high level in open water be excluded, no other such aggregation of green fishes as may be secured on the grass flats may be named from the entire fish fauna of the region, though this includes more than two hundred species.

It seems significant that the suggestion from the observations so far recorded is uniform. The obliterative effect of countershading is demonstrated by Thayer's experiments. That adaptive color changes are very common and minister to the same end, although they occur among bright colored species, is a fair inference from my own experience. The colors of the fishes, also, are correlated with their habits in such

a way that, upon the average, their conspicuousness would apparently be increased if their pigments were very different from those they display.

Additional research demonstrates that in so far as this class of animals is concerned there is no ground for the belief that bright color is correlated in any way with armament or distastefulness. If a list of species possessing organs capable of inflicting painful bodily injury be compiled, their colors and ability to change them are found to differ in no essential respect from those of any other group of the same size selected at random. That no unpalatability is correlated with gaudy coloration is proved by the fact that forms unsurpassed in brilliancy constitute an important part of the food, and may be recovered regularly from the stomachs of snappers, which are among the commonest of the predaceous fishes of the Tortugas. Hence, in view of all the evidence, it seems improbable, to say the least, that the bright colors of some fishes differ in function from the dull hues of their more modest congeners. But since the most highly colored of these creatures vie with birds and butterflies in vividness of coloration, it becomes necessary to revise all hypotheses which postulate conspicuousness.

It is interesting to find that mimicry among insects may be explained much more consistently than is otherwise possible upon the assumption that even the colors of the most gaudy tend to reduce their visibility. Every authenticated fact adduced by the supporters of the mimicry hypotheses may be accounted for, and most, if not all of the criticism levelled against them may be met, if the matter be set forth as follows:

Mimicry has arisen through bionomic pressure applied first by indiscriminate feeders, which have forced upon their accustomed prey color combinations which most effectually conceal it in its normal environment. In addition, by chance, in a few of many thousands of cases in which colors appropriate to the surroundings and habits of their possessors have been evolved, patterns have appeared, sufficiently like one another to deceive enemies that exercise discrimination in their choice of food. From this point onward the evolution of resemblance has proceeded according to accepted formulae, without conspicuousness being involved in the process.

We may assume that the *Pieridae* and *Heliconidae*, for example, are usually distinctly different in habit, and that the coloration of typical members of each family is a combination of hues well suited upon the average to render them inconspicuous in such places as they frequent. If this be so, the initial step toward mimicry might be any one of many

variations in nutrition or reproduction which would lead representatives of the first family to live after the manner of the second. Reason has already been given for supposing that convergence in color would accompany convergence in habit.

Whatever may eventually prove to be the case with the problem of mimetic resemblance, the observations presented in this abstract embody a great mass of fact whose theoretical significance is obvious. It undermines many speculative explanations of animal coloration in terms of natural selection, but, being itself consistent with the Darwinian hypothesis, it replaces them by something which may not be lightly dismissed from consideration. It emphasizes the common occurrence among animals of attributes of apparent advantage to them, and forces the issue between natural selection and the inheritance of acquired characters as the immediate cause of adaptation.

NOTICES OF BIOGRAPHICAL MEMOIRS

The following biographical memoirs have been published by the Academy since the last notices of such memoirs appeared in the November, 1915, number of the PROCEEDINGS.

GEORGE WILLIAM HILL (1838–1914). By ERNEST W. BROWN. *Biographical Memoirs of the National Academy*, **8**, pp. 275–309.

This Memoir discusses the life-work of George W. Hill along the following outline: Boyhood, First papers; Influence of Delaunay and Hansen, Comet of 1858, Elements of Venus, the Years 1872–1875; the Great Decade 1875–1885, Hill's Mental Development, Astronomy before Hill, the Two Great Memoirs of 1877, Influence of Euler, the Periodic Orbit, Stability, Infinite Determinants, Relation to J. C. Adams' Work, Theories of Jupiter and Saturn, Estimate by F. R. Moulton; Residence in Washington, Application of the Methods of Delaunay, Hansen, de Pontécoulant, and Gauss; Concluding years, Hill's Characteristics, Estimates by Poincaré, R. S. Woodward, A. S. Flint, H. B. Hedrick, and H. Jacoby; Hill's Scientific Honors; Bibliography.

THEODORE NICHOLAS GILL (1837–1914.) By WILIAM HEALEY DALL. *Biographical Memoirs of the National Academy*, **8**, pp. 313–343.

This Memoir recounts the life-work of Theodore N. Gill: Boyhood, Report on the Fishes of New York, Trips to the Antilles and Newfoundland; Connections with the Smithsonian Institution, the Library of Congress, and the U. S. Fish Commission; Editor of the *Osprey* Work on Mollusca; Estimates by the Commissioner of Fisheries, by the Director of the National Museum; Associations with George Washington University; Personal Characteristics; Bibliography.

NATIONAL RESEARCH COUNCIL

REPORT OF THE SECOND MEETING OF THE COUNCIL

The second meeting of the Research Council was held on November 13 and 14, 1916, in Cambridge, in connection with the Autumn Meeting of the National Academy of Sciences.

No formal actions were taken.

Dr. W. H. Welch, President of the Academy, reviewed the organization and earlier work of the Council; and Dr. G. E. Hale, chairman of the Council, reported on the recent activities of its committees, and discussed broadly the general purposes of the movement and the plans for the immediate future. He described in some detail the studies which he has recently made in England and France of the organizations of scientific men in those countries for the purpose of assisting their governments.

Addresses were made by members of the Council as follows:

Dr. S. W. STRATTON, Director of the National Bureau of Standards, Washington. Target practice in the Navy and some of the research problems involved; illustrated with moving pictures.

Lieut. Col. GEORGE O. SQUIER, Chief of Aviation, U. S. Army. Scientific research for national defense, as illustrated by the problems of aviation.

Dr. ARTHUR A. NOYES, Massachusetts Institute of Technology. The nitrogen problem in war and in agriculture.

Various phases of the work of the Council were then discussed by several of its members.

CARY T. HUTCHINSON, *Secretary.*

REPORT OF MEETINGS OF THE EXECUTIVE COMMITTEE

The third meeting of the Executive Committee of the Research Council was held in New York City on October 16, 1916. Messrs. Carty, Conklin, Dunn, Hale, Pearl, Pupin, Stratton, and the Secretary were present.

The following resolutions adopted by the American Philosophical Society on October 6, 1916, were presented to the Committee:

WHEREAS as effort is being made to bring into co-operation existing governmental, educational, industrial, and other research organizations with the object of encouraging the investigation of natural phenomena, the application of scientific principles in American industries, the employment of science in the national defense, and such other objects as will promote the national welfare; and

WHEREAS, these objects are among those for which the American Philosophical Society exists.

Now, therefore, be it resolved that the American Philosophical Society hereby registers its approval of the co-ordination and federation of the research agencies of the country and expresses its willingness to join with and assist the National Research Council, organized by the National Academy of Sciences to accomplish the above federation.

A letter was presented from the National Canners' Association asking the co-operation of the Research Council in the conduct of investigations on toxic elements in canned foods: Raymond Pearl, V. C. Vaughan, and C. L. Alsberg were appointed a committee to confer with representatives of the Association in regard to the matter.

The matter of the ice patrol of the North Atlantic was presented by Dr. S. W. Stratton, and it was voted that a committee be appointed by the Council to co-operate with the government Committee on Ocean Surveys and Ice Patrol.

The question of agricultural research in this country was presented by Dr. Raymond Pearl and discussed by the committee. It was voted that Dr. Pearl and Dr. Hale be authorized to confer with the Secretary of Agriculture to ascertain from him how best the Research Council can co-operate with his Department and who would best represent his Department in the work of the Research Council.

Regarding the procedure of the committees, it was voted that the committees of the Council appointed for carrying out the various branches of the work first decide upon a general plan of procedure and submit this plan to the Executive Committee for its approval. The committees shall then proceed to carry on in detail the general plan as approved by the Executive Committee; but they shall not take any action connected with legislation or with questions of public policy without first consulting the Executive Committee. They shall make reports of progress to the Council at its annual meeting and to the Executive Committee at such times as it may direct.

The fourth meeting of the Executive Committee was held on October 21, 1916, in New York City. Messrs. Carty, Dunn, Hale, Noyes, and the Secretary were present.

A committee on the Utilization of Industrial Research Facilities for the National Defense was formed, and J. J. Carty was appointed chairman.

The rest of the meeting was devoted to a consideration of a report drafted by Dr. G. E. Hale to be presented to the President of the Academy on the work of the Research Council and to be transmitted by him to the President of the United States.

The fifth meeting of the Executive Committee was held on November 4, 1916, in New York City. Messrs. Carty, Dunn, Hale, Noyes, and the Secretary were present. Dr. Hale reported that under the authorization previously given by the Executive Committee he had proceeded with the organization of the Military Section of the Research Council. As members of that Section there have been appointed by the President of the United States three members from the Army, namely, Brigadier-General William Crozier, Chief of Ordnance; Brigadier General W. C. Gorgas, Surgeon General; and Lieut. Col. G. O. Squier, Chief of Aviation, and three representatives of the Navy,

namely, Rear-Admiral D. W. Taylor, Chief Constructor; Rear-Admiral R. S. Griffin, Engineer-in-Chief; and Dr. J. D. Gatewood, Medical Director.

It was voted to appoint a committee consisting of V. C. Vaughan (chairman), C. B. Davenport, H. H. Donaldson, W. H. Holmes, and Raymond Pearl to consider the organization of research in anthropological and sociological sciences, and to report to the Executive Committee in regard to this matter.

A report was presented from the Chemistry Committee in regard to appointment of sub-committees. It was voted that such committees be appointed by the Research Council on nomination of the Chemistry Committee.

The sixth meeting of the Executive Committee was held on November 15, 1916, in Boston. Messrs. Conklin, Hale, Noyes, Pearl, Stratton, Vaughan, and Welch were present.

A report was presented from the Committee on Chemistry (M. T. Bogert, chairman) recommending the organization of a number of sub-committees. It was voted to defer action on these committees until the relation of certain of them to other branches of science could be properly considered.

The Committee then took up the consideration of the formation of general committees representing various branches of science. It was voted to organize the following general science committees (in addition to that on Chemistry previously established): Mathematics. Astronomy. Physics. Geology and Paleontology. Geography. Botany. Zoology and Animal Morphology. Physiology. Medicine. Hygiene. Agriculture. Psychology. Anthropology.

CARY T. HUTCHINSON, *Secretary.*

NATIONAL RESEARCH COUNCIL

SCIENTIFIC RESEARCH FOR NATIONAL DEFENSE AS ILLUSTRATED BY THE PROBLEMS OF AERONAUTICS

Abstract from an Address of Lieut. Colonel George O. Squier, U. S. A.

The following are some present problems connected with the development of Military Aviation and Aerostation.

1. *Aerodynamics.*—(*a*) Continue the development of the mathematical theory to explain the aerodynamic phenomena recorded in the aerodynamical laboratories, and to forecast further results.

(*b*) Obtain solutions for the speed and direction of flow of air about geometric and aerotechnic forms and develop experimental means to visualize or map the speed and direction of flow.

(*c*) Map the currents of the upper atmosphere which may be of most use in aerial navigation, and evolve simple practical rules for the guidance of pilots.

(*d*) Give fuller explanation of the phenomena of soaring, i.e., airplaning indefinitely without motive power.

(*e*) Develop equations and laws of comparison by which the behavior of large aircraft may be more accurately foretold from tests of models. Apply further the principle of dynamical similarity.

(*f*) Investigate more direct and effective methods of securing a lift or thrust in the air from the consumption of fuel.

(*g*) Complete theory of the air-screw.

2. *Engine problems requiring research.*—(*a*) Fuel. Possibly the most far-reaching problem is fuel. A fuel that will carry more power into an engine per unit volume will be a direct gain.

Attempts have been made to combine alcohol, gasoline, acetylene, picric acid, ether and other hydrocarbons with the above object in view. Questionable results have followed. There has been an increase of power, but nothing so far commercially or practically useful.

This question must be studied with the greatest of care and from a truly research standpoint.

(*b*) Solid fuel. Solid fuels that can be converted into liquid in small quantities just prior to use, are desirable for military aviation. In case of accident from shot or shock, leakage of liquid fuel is a danger. Solid fuel could be carried in quantity with less danger.

(*c*) Engine cooling. The problem of radiation is important. If some substance could be found that would circulate through the cooling system, at higher temperatures than water, it is probable that greater engine efficiencies would result. Oils, salt waters, and other materials have been tried with indifferent success.

(*d*) Liquid fuel pipes. Tubing that will resist vibration (causing rupture) is desired. An oil and gasoline proof rubber tubing is reported as used in Europe. This development is highly important, not only for tubing, but for containers in which to carry liquid fuel. Some sort of fabric and rubber tank that would really resist the action of gasoline, would be of the highest benefit.

A difficulty lies in the fact that the tanks are large (say 20 to 100 gallon capacity). The structural problems would be serious. The tanks now used are large and of metal. Vibration causes much difficulty and leakage.

(*e*) Metal coating. The protecting of the metal parts of an airplane, especially the fittings and cables, is a serious problem. A material is desired that would really prevent dangerous corrosion. Nickel plating over copper is very good, but will not suffice. Rust strikes through very rapidly. Baked enamel is the best coating. It is impossible to apply in many cases.

(*f*) Sound. The question of eliminating the noises involved in the operation of aircraft is one of importance. The peculiar note of the propeller of a Zeppelin can be heard for several miles, and is usually the first warning of its approach at night.

3. *Miscellaneous.*—(*a*) Physiological. Study the physiological and psychological effects of low density air at high altitudes on the performance of pilots.

(*b*) Transparent wing covering for airplanes. A wing covering which

would answer the following general requirements would be of great value to military aviation:

Weight not more than 5 ounces per square yard.

It should present reasonably great resistance to flame.

It should be reasonably proof against action of salt water, moist air, extreme dryness, and quick temperature changes.

It should not stretch in any direction. Its ability to retain original form as placed on the airplane is very important.

It should have tensile strength of at least 75 pounds per inch width in any direction.

Its tendency to tear and split because of tack holes through it, or because of bullet holes, should be as small as possible.

(*c*) Development of light alloys for airplane construction. Pure aluminium or aluminium alloys. It is believed that a great deal can be done in this direction. So far no alloy has been developed, except possibly in Germany, which can compare with average Alaskan spruce in its 'specific tenacity.'

(*d*) The structure of gusts. It is believed that this is of sufficient importance to aviation to warrant considerable expense in its study.

Painstaking investigation of the character of eddy formations caused when wind strikes trees, hollows, cliffs, etc., and the character of disturbances created by canyons, swamps, deserts, etc., would be of great value to aviators.

This can be done not only by smoke and toy balloon work in the vicinity of obstructions such as the above, but also by photographic work in wind channels.

A set of simple rules laying down just what the aviator may expect on one side or another of canyons, cities, trees, lakes and swamps would be very helpful in aviation.

(*e*) Radio-apparatus for aircraft. The subject of radio-intercommunication between aircraft in flight, and between aircraft and the earth requires for its solution the highest possible efficiency and reliability combined with minimum weight.

A present tendency is to separate entirely the power plant from the main engine of an aircraft. The generator body in this case has a stream-line figure and a separate small air-screw is provided. Among other methods the oscillion is being tried as the actual source of continuous electromagnetic waves.

(*f*) Bullet proof gasoline tanks. Development of a material with which to line or construct tanks to contain the gasoline in an airplane in which a bullet hole will quickly close, entirely or partly at least. This would enable many a flier to get back to his own lines after having been fired upon.

(*g*) Development of a fabric as good as, or better than, Irish linen, for the covering of airplanes. There has not been manufactured in this country a fabric suitable for use in covering airplanes.

The fabric should answer all requirements laid down under *transparent wing covering*, and be, in addition, such as to shrink the proper amount without harm when cellulose solution is applied.

It is possible that long fiber cotton might be developed that would answer the purpose.

We must become independent in all lines affecting our military aviation. Today we depend entirely upon Ireland and England for our linen, and the supply is becoming very low in this country.

(*h*) Aviator's clothing. Much is still to be done in devising non-inflammable and protective clothing for aviators. This question is intimately connected with personal armor and safety in case of fall.

(*i*) Ground-speed indicator. An instrument which would measure the actual speed of an aircraft over the ground would be useful in the operation of military machines.

4. *Physics of the Air.*—A number of physical properties of air, important in the problems of avaiation, were also discussed.

RESEARCH GRANTS FROM THE TRUST FUNDS OF THE ACADEMY

Since the last report, these PROCEEDINGS **2**, 307, the following grants for the promotion of research have been made from the Trust Funds of the Academy.

GRANTS FROM THE BACHE FUND

No. 196, H. N. NORRIS, $100. For assistance in making numerous preliminary sketches and drawings, involved in the plotting from serial sections, the immediate work to be carried on with Cryptobranchus material. An investigation of the cranial nerves of the Amphibia, involving first, an analysis of the nerve components of representative and typical Modela, on the plan followed in the writer's papers on the cranial nerves of Amphinnea and Siren; second, a review of the nerve component in the Anura. Finally a monograph on the cranial nerves of the Amphibia as a whole.

No. 197, G. P. BAXTER, Harvard University, $300. For the purchase of platinum and quartz apparatus, instruments of precision, special glass apparatus and glass blowing, and special chemicals for the determination of atomic weights; namely, of arsenic and iodine, by iodimetric comparison of arsenic trioxide with iodine and iodine pentoxide, and of various metals by electrolysis of weighed amounts of salts, and by coulometric comparison.

No. 198, L. T. MORE, University of Cincinnati, $500. For the purchase of a high voltage (200,000 volts), large current (10 kilowatt) generator of the transformer synchronous commutator type, to examine the region of the radiation spectrum between the hardest known X-rays and the X-rays of radium and the characteristic K radiation and its absorption by various metals.

No. 199, F. P. REAGAN, $100. For an intensive study of haematopolsis in teleost embryos involving continual observation.

No. 200, H. W. NORRIS, Grinnell College, $250. For an investigation of the cranial nerve components of the common dogfish, squales, acanthias; an analysis of the cranial ganglia and the peripheral distribution of the component fibers.

No. 201, A. F. SHULL, $400. To discover the causes of the changes in the life cycles of rotifers, aphids, and thrips, with special reference to the determination of sex; also related cyclical phenomena in the aphids, primarily the sporadic production of wings.

GRANT FROM THE J. LAWRENCE SMITH FUND

No. 7, GEORGE PERKINS MERRILL, U. S. National Museum, $500. To further aid his studies of rare meteorites.

GRANT FROM THE MARSH FUND

JOHN M. CLARK, State Hall, Albany, $400. For a study of mutualism, symbiosis, and dependent life among animals of geologic time.

REPORT OF THE AUTUMN MEETING

Prepared by the Home Secretary

The Autumn Meeting of the Academy was held in the buildings of the Massachusetts Institute of Technology at Cambridge, on November 13, 14, 15, 1916.

Seventy members were present as follows: Messrs. C. G. Abbot, H. L. Abbot, Barus, Baxter, Becker, Boas, Bogert, Boltwood, Bumstead, Cannon, Castle, Cattell, Chittenden, W. B. Clark, J. M. Clarke, Comstock, Conklin, Crafts, Crew, Cross, Davenport, Davis, Day, Farlow, Fewkes, Folin, Goodale, Hale, E. H. Hall, Harper, Harrison, Hayford, Holmes, Howard, Howell, Iddings, Jackson, Leuschner, Lewis, Lindgren, Lusk, Mall, Mark, Mayer, Meltzer, Mendel, Michelson, Morley, E. S. Morse, A. A. Noyes, H. F. Osborn, T. B. Osborne, Parker, Pearl, Pickering, Prudden, Reid, Richards, Rosa, Scott, Story, Thomson, Van Hise, Walcott, Webster, Welch, Wheeler, D. White, Wilson, R. W. Wood.

BUSINESS SESSIONS

The Home Secretary announced the following deaths since the last Annual Meeting of the Academy: Cleveland Abbe, elected 1879, died October 28, 1916; Josiah Royce, elected 1906, died September 14, 1916; also Sir William Ramsay, Foreign Associate, elected 1904, died July 24, 1916.

The Home Secretary announced that the following publications had been issued since the Annual Meeting: PROCEEDINGS, vol. 2, numbers 5–11; *Memoirs*, volume 14, first memoir, by George P. Merrill, consisting of a Report on Researches on the Chemical and Mineralogical Composition of Meteorites, with Especial Reference to Their Minor Constituents; Biographical Memoirs of George William Hill by Ernest W. Brown, and of Theodore N. Gill by William H. Dall.

Under the rules of the Academy the following members of the Editorial Board of the PROCEEDINGS retire on December 1, 1916: W. B. Cannon, J. McK. Cattell, J. M. Coulter, S. Flexner, R. G. Harrison. The Home Secretary announced that the following members have been appointed by the Council to serve in their places until December 1, 1919; H. H. Donaldson, R. A. Harper, Graham Lusk, A. G. Mayer, A. A. Noyes. Raymond Pearl was appointed to succeed A. A. Noyes as Chairman of the Editorial Board.

The Home Secretary also announced that an Auditing Committee has been appointed consisting of W. H. Dall, chairman, A. L. Day, and F. W. Clarke;

and that a Committee on the Organization of the Scientific Resources of the Country for National Service was appointed by the Council as follows: G. E. Hale, chairman, Simon Flexner, E. G. Conklin, R. A. Millikan, A. A. Noyes. This last Committee presented a detailed report recommending the organization of the National Research Council.

The Home Secretary also announced that a Committee on Nitrate Supply was appointed at the request of the Secretary of War to present to the War Department "a report containing such information as will aid the department, to the greatest degree which the Academy finds practicable, in reaching a conclusion as to the best method to be followed in the manufacture of nitric acid by a process not involving dependence upon a foreign source of supply, the report to take into consideration the legislation contained in Section 124 of the National Defense Act approved June 3, 1916." The following men were appointed members of this Committee by the President of the Academy: Arthur A. Noyes, chairman, Leo H. Baekeland,* Gano Dunn, Charles H. Herty,* Warren K. Lewis,* Michael I. Pupin, Theodore W. Richards, Elihu Thomson, Willis R. Whitney.* Those against whose name an asterisk is placed were named by the President of the American Chemical Society.

The President announced that he had forwarded a preliminary report on the National Research Council to the President of the United States.

The following new members were presented to the Academy: M. T. Bogert, L. O. Howard, A. G. Mayer, Raymond Pearl.

The Treasurer reported that notice had been received from the attorneys for Mr. W. W. Farnam, the executor of the estate of Othniel C. Marsh, that he had filed in the New Haven probate court a supplemental account as executor which showed a balance of interest due the Academy of $6,921,84.

The resolution in regard to a section of engineering which was proposed by the Council and adopted at the Business Session of the Academy on April 17, 1916, was presented, and it was voted that a copy of the resolution be sent to each member of the Academy. The resolution is as follows:

Voted that a section of engineering be developed which shall include men who have made original contributions to the science or art of engineering; that to this end the present section of physics and engineering be designated the section of physics, and that the Council, under the authority granted by section 4, article 4, of the Constitution, nominate to the Academy, after inviting suggestions from the members of the Academy, two or three engineers each year until such time as it shall seem advisable to establish a separate section of engineering, any engineers elected as the result of such nominations being in the meantime assigned to that one of the existing sections to which their work is most closely related.

The following amendments to the Constitution reported from the Council and returned to the Academy from the Committee of the Whole with favorable recommendation were considered:

That article 3, section I, line 3, of the Constitution be amended by substituting the word "fourth" for "third" in the phrase specifying that the Annual meeting shall begin "on the third Monday in April."

That the last sentence of section 5, article 4, of the Constitution be stricken out.

The first of these amendments was rejected and the second one was adopted.

A new amendment to the Constitution was offered providing that article III, section I, in regard to the meetings of the Academy be amended so as to read as follows:

> Article III, section 1. The Academy shall hold one stated meeting in each year, called the annual meeting, in the city of Washington and another, called the autumn meeting, both to be held at such place and time as the Council shall determine, provided that the annual meeting shall be held in April.

This proposed amendment was referred to the Council.

Mr. C. R. Van Hise presented the report of the Committee on the Panama Canal Slides. It was voted that the printing of the final report be referred to the Council with power.

It was voted that the Home Secretary be directed to send copies of the minutes of each Business Session of the Academy to all members of the Academy for approval and correction.

It was voted that papers by non-members appearing in the PROCEEDINGS be announced as 'communicated by' some member.

It was voted on recommendation of the Committee on the J. Lawrence Smith Fund that a grant of $500 be made from that fund to Mr. George Perkins Merrill to further aid his studies of rare meteorites.

It was voted on recommendation of the Committee on the Marsh Fund that a grant of $400 be made from that fund to Mr. John M. Clark for a study of mutualism, symbiosis, and dependent life among animals of geologic time.

SCIENTIFIC SESSIONS

Two public lectures were given on November 13, by President W. H. Welch on The Formation of the National Research Council at the Request of the President of the United States; and by S. W. Stratton, Director of the National Bureau of Standards, Washington, on Target Practice in the Navy and Some of the Research Problems Involved, illustrated with moving pictures. There followed a public Scientific Exhibit (see list of exhibits, below).

At a joint session of the Academy and of the National Research Council on November 14 three public lectures were given by George E. Hale, Chairman of the National Research Council, on The Work of the National Research Council and on Recent Observations of Organized Science in England and France; by Lieut. Col. George O. Squier, Chief of Aviation, U. S. Army, on Scientific Research for National Defense, as illustrated by the Problems of Aviation; and by Arthur A. Noyes, Massachusetts Institute of Technology, on The Nitrogen Problem in War and in Agriculture.

Two public scientific sessions were held on November 13 and 14 at which the following papers were presented (an asterisk denotes presentation only by title):

RAYMOND PEARL, Maine Agricultural Experiment Station: Some effects of the continued administration of alcohol to the domestic fowl, with special reference to the progeny.

ALFRED G. MAYER, Marine Laboratory, Carnegie Institution: Further studies of nerve conduction.

E. G. CONKLIN, Princeton University: The share of egg and sperm in heredity.

LAFAYETTE B. MENDEL and S. E. JUDSON, Yale University: Some inter-relations between diet, growth, and the chemical composition of the body.

CHARLES P. OLIVIER, University of Virginia (read by E. W. Morley): The meteor system of Pons-Winnecke's comet. 129 parabolic orbits of meteor streams.

ALESSANDRO FABBRI (introduced by A. G. Mayer): Micro-cinematographs of marine and freshwater organisms.

A. G. WEBSTER, Clark University:* Practical tests of a new phonotrope.

EDWARD S. MORSE, Salem, Mass:* Protoconch of Solemya.

JACQUES LOEB, Rockefeller Institute:* Diffusion and secretion.

HENRY L. ABBOT, Cambridge, Mass:* Hydrology of the Isthmus of Panama.

JOHN M. CLARK, State Museum, Albany:*The Strand and the Undertow.

W. LINDGREN, Mass. Inst. of Tech:. *Notes on the deposition of quartz, chalcedony, and opal.

W. M. DAVIS, Harvard University:*Sublacustrine glacial erosion in Montana.

H. F. OSBORN, American Museum of Natural History: *The tyrant (Tyrannosaurus) and the ostrich (Struthiomimus) dinosaur. The relation of Newton's and Darwin's laws to the fundamental biologic law.

EDWIN H. HALL, Harvard University: Electric conduction in metals.

EDWARD B. ROSA, National Bureau of Standards: The silver voltameter as an international standard.

R. W. WOOD, Johns Hopkins University: One-dimensional gases and the reflection of molecules. Series in resonance spectra.

ELIHU THOMSON, Swampscott, Mass.: Inferences concerning auroras.

A. A. MICHELSON, University of Chicago: Report of progress in experiments for measuring the rigidity of the earth. The laws of elastico-viscous flow.

C. G. ABBOT, Smithsonian Institution: On the preservation of knowedge.

FRANZ BOAS, Columbia University: Further evidence regarding the instability of human types.

ROSS G. HARRISON, Yale University: Transplantation of limbs.

CHAS. B. DAVENPORT, Station for Experimental Evolution, Carnegie Institution: Heredity of stature.

F. R. MOULTON, Chicago University: *On analytic functions of infinitely many variables.

HENRY S. WHITE, Vassar College, F. N. COLE and LOUISE D. CUMMINGS:* Enumeration of all triad systems on fifteen elements.

WILLIAM E. STORY, Clark University: *Some variable 3-term scales of relation.

EDW. L. NICHOLS, Cornell University: *New data on the phosphorescence of certain sulphides.

G. P. BAXTER and H. W. STARKWEATHER, Harvard University: *A revision of the atomic weight of tin.

T. W. RICHARDS and H. S. DAVIS, Harvard University: *Improvements in calorimetric combustion.

T. W. RICHARDS and C. WADSWORTH, 3d, Harvard University: *Further study of the atomic weight of lead of radioactive origin.

GILBERT N. LEWIS, University of California. *Chemical affinity.

WM. TRELEASE, University of Illinois: *The American oaks.

H. S. JENNINGS, Johns Hopkins University: *The numerical results of diverse systems of breeding, with relation to two pairs of factors, linked or independent.

W. R. MILES, Carnegie Nutrition Laboratory, Boston (introduced by F. G. Benedict): *Some psycho-physiological processes as affected by alcohol.

WALTER B. CANNON, Harvard University: *Oscillatory variations in the contraction of rhythmically stimulated muscles.

WM. H. DALL, Smithsonian Institution: *On some anomalies in the distribution of Pacific coast mollusca.

G. H. PARKER, Harvard University: *The responses of hydroids to gravity.

W. M. WHEELER, Harvard University: *The phylogenetic development of subapterous and apterous castes in the Formicidae.

W. J. CROZIER, Resident Naturalist, Bermuda Biological Station (introduced by E. L. Mark): *On cell penetration by acids: the chloracetic acids. *On the immunity coloration of some nudibranches.

HOVEY JORDAN, Bermuda Biological Station (introduced by E. L. Mark): *The rheotropism of the marine fish known as "hamlet" or "grouper" (*Epenephalus striatus*).

A. C. WALTON, Harvard University (introduced by E. L. Mark): *The occurrence of *Ascaris triquetra*, Schrank, in dogs.

The following is the list of scientific exhibits prepared for the meeting of the Academy.

H. S. WHITE, Vassar College: Graphic representations of triad systems.

MISS A. J. CANNON, Harvard College Observatory: Stellar spectra.

LEON CAMPBELL, Harvard College Observatory: Visual observation of variable stars.

MISS H. S. LEAVITT, Harvard College Observatory: Photographic magnitudes.

SOLON I. BAILEY, Harvard College Observatory: Variable stars in clusters.

A. G. WEBSTER, Clark University: Acoustical measuring apparatus: standard phone, phonometer and phonotrope. Application of a drop chronograph for use in ballistics.

CHARLES A. KRAUS, Clark University: A new vacuum pump and a new thermostat.

H. P. HOLLNAGEL, Mass. Inst. Technology: Methods of isolating the infra-red region of the spectrum.

ALEXANDER MCADIE, Blue Hill Observatory: Cloud studies, wind structure and snow flakes.

ELLSWORTH HUNTINGTON, Milton, Mass.: The relation between solar changes and barometric gradients. Optimum temperature for the human race.

ROBERT DEC. WARD, Harvard University: Weather types of the United States, illustrated by composite weather maps and instrumental records.

R. A. DALY and H. CLARK, Harvard University: Design for a deep-sea thermograph.

FRANK HALL, Mass. Inst. Technology: A thermophone arranged so that direct comparison may be made with a magnetic receiver.

A. H. GILL, Mass. Inst. Technology: Tests of lubricating mineral oils.

F. G. KEYES and J. B. DICKSON, Mass. Inst. Technology: Continuous flow calorimeter for measuring heats of reaction in solution.

C. L. BURDICK, Mass. Inst. Technology: Determination of crystal structure by X rays.

R. E. WILSON, Mass. Inst. Technology: Apparatus for maintaining pressures of one-tenth micron or less, and the investigation of the mechanism of chemical reactions.

HENRY FAY, Mass. Inst. Technology: Erosion of large guns.

ALBERT SAUVEUR, Mass. Inst. Technology and Harvard University: 1. Photomicrographic apparatus (original). 2. Photomicrographs of metals and alloys; charts and diagrams; specimens.

H. O. HOFMAN, Mass. Inst. Technology: 1. Jenny flotation machine. 2. A laboratory revolving horizontal roasting furnace heated electrically and rotated in the same way.

A. E. KENNELLY and Associates, Mass. Inst. Technology: Researches in electrical engineering.

ALEXANDER KLEMIN, Mass. Inst. Technology: Aeroplane models used in wind tunnel.

W. LINDGREN and W. L. WHITEHEAD, Mass. Inst. Technology: Photomicrographs of silver ores from Chile and Tintic.

C. H. WARREN, Mass. Inst. Technology: 1. A graduated sphere for crystallographic work. 2. Photographs of spherulites in polarized light.

CHARLES PALACHE, Harvard University: Models showing gnomonic crystal projection.

WALLACE W. ATWOOD, Harvard University: The former glaciers of the San Juan mountains of Colorado. The physiographic stages in the evolution of the San Juan mountains of Colorado.

J. B. WOODWORTH, Harvard University: Glacial map of Cape Cod and adjacent islands. A glypolith from Nantucket.

LAURENCE LA FORGE, U. S. Geological Survey: Recent topographic and geologic maps of New England and other parts of the United States.

U. S. COAST and GEODETIC SURVEY: Recent charts of the coasts of the United States.

JOHN M. CLARKE, State Museum, Albany, N. Y.: Portfolio of paleontological plates, in press. Plates of "Wild flowers of New York," in press. Geological map of Ogdensburg, N. Y., and vicinity, in press.

H. W. SHIMER, Mass. Inst. Technology: Evolution of some brachiopods.

RICHARD M. FIELD, Harvard University: Ordovician rocks and faunas of central Pennsylvania.

W. B. SCOTT, Princeton University: Proofs of plates for forthcoming report on paleontology of Patagonia.

W. J. V. OSTERHOUT, Harvard University: Pigments produced by the oxidation of a colorless plant chromogen.

CHARLES W. JOHNSON, Boston Society of Natural History: Distribution and variation of Helix hortensis.

JOSEPH A. CUSHMAN, Boston Society of Natural History: Some fossil and recent foraminifera.

ALFRED G. MAYER, Marine Laboratory, Carnegie Institution: Yacht and laboratory of the Carnegie Institution at Tortugas, Florida.

HUBERT LYMAN CLARK, Museum of Comparative Zoölogy, Harvard University: Echinoderms from Torres straits, Australia, with colored drawings and lithographs.

G. H. PARKER, Harvard University: The suction efficiency of a Californian sea-anemone.

W. T. BOVIE, Harvard University: Visible effects of Schumann rays on protoplasm. Effects of radium rays on permeability of protoplasm.

C. T. BRUES, Bussey Institution, Harvard University: Specimens and charts illustrating insects as carriers of infantile paralysis.

W. E. CASTLE, Bussey Institution, Harvard University: Examples of Mendelian inheritance, reversion and variety formation in rats and guinea-pigs.

FRANCIS G. BENEDICT, Nutrition Laboratory, Carnegie Institution: Respiration apparatus for animals.

T. B. OSBORNE, Connecticut Agricultural Experiment Station, and L. B. MENDEL, Sheffield Scientific School, Yale University: Photographs representing the growth of chickens fed with definite mixtures of food stuffs under laboratory conditions which have heretofore not led to success.

I. CHANDLER WALKER, Medical Service, Peter Bent Brigham Hospital: Proteid sensitization in relation to bronchial asthma.

H. S. WELLS, Medical Service, Peter Bent Brigham Hospital: Electrocardiography, or the application of the string galvanometer to the study of cardiac cases.

ALBERT A. GHOREYEB, Cancer Commission, Harvard University: Metal casts of heart and kidney blood vessels.

S. B. WOLBACH, Harvard Medical School: Studies in Rocky mountain spotted fever.

HARVEY CUSHING and W. M. BOOTHBY, Peter Bent Brigham Hospital: Apparatus of routine methods for clinical metabolism determinations.

E. W. GOODPASTURE, Peter Bent Brigham Hospital: An anatomical study of senescence, with especial reference to tumors.

E. E. TYZZER and C. C. LITTLE, Harvard Medical School: The inheritance of susceptibility to transplanted tumor.

W. DUANE, Harvard Medical School: The technique of the preparation of radium for therapeutic purposes.

G. C. WHIPPLE, School for Health Officers, of Harvard University and Mass. Inst. Technology: Charts showing organization and membership of the School.

W. T. SEDGWICK, Mass Inst. Technology: 1. Diagrams and tables illustrating the investigations of Professor Weston and Mr. Turner upon "The digestion of sewage effluents in an otherwise unpolluted stream." 2. An investigation of the behavior of certain species of bacteria in various materials between zero Centigrade and zero Fahrenheit. 3. A field investigation of the sanitary environment of a suburban population.

S. C. PRESCOTT, Mass. Inst. Technology: Diseases of the banana in Central America and their control.

ALFRED M. TOZZER, Peabody Museum, Harvard University: Race-mixture in Hawaii.

CHARLES PEABODY, Peabody Museum, Harvard University: Prehistoric specimens from caves of France and Palestine.

E. A. HOOTON, Peabody Museum, Harvard University: Casts and reconstruction of ancient man skull of apes.

S. J. GUERNSEY, Peabody Museum, Harvard University: Cave explorations in northeastern Arizona.

ORIC BATES, Peabody Museum, Harvard University: Prehistoric Libyan remains.

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